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AN EXPERIMENTAL STUDY OF VISUAL FIXATION

BY

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AN EXPERIMENTAL STUDY OF VISUAL FIXATION.

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CHAPTER I. THE FIXATION FIELD.

§ 1. *Visual Fixation and Its Anomalies.*

Every complete visual act involves both eye movement and fixation. The two factors alternate when eye movements of the 'first type'[1] (*i. e.*, simple rapid eye movement) separate moments of significant stimulation. They are concurrent in compensatory and pursuit movements. And finally, they are complexly related in the relatively slow movements of convergence. Whatever their mutual relationship, however, in normal vision it would be absurd to regard either one or the other as an end in itself. Both eye movement and fixation are means to the attainment of a satisfactory visual impression; and since both are the products of muscular activity, fixation, even if it were absolute, would be only a limiting instance of eye movement, while the study of the latter finds its natural complement in a study of the consequent fixation.

The close relationship between eye movement and fixation is reflected in the uncertainty of traditional terminology. The term 'fixation' is frequently used to cover the entire process of visual adjustment, including the antecedent eye movement. In general, however, fixation appears to mean that the point of regard remains relatively unchanged within the visual field. In this sense the point of regard is commonly called the fixation point. It is obviously in this sense that I have used the term. On the other hand it is a commonplace of physiological optics that the globe of the eye is never fixed in the sense that it is free from minute irregular movements. The latter become

[1] *American Journal of Physiology*, Vol. VIII., 301, fol.

distressingly obvious when one attempts quantitative observations on the living globe. In general, however, these irregular eye movements have been regarded as accidental variations from a normal constant fixation, and the latter has been somewhat uncritically regarded as the type. Recent investigations of the eye movements, however, justify the assertion that absolute fixation of the globe does not exist in normal vision.

The first systematic study of these minute involuntary eye movements with which I am acquainted was made by Delabarre. Unquestionably the most complete account of the irregularities of fixation was published by McAllister in the Yale Studies.[1] Both investigators found that during supposed fixation there is continuous eye movement over a variable area of very appreciable extent.

McAllister found that the amplitude of these involuntary eye movements varies with the character of the object of regard and its surroundings. It also varies with the individual, and is different for the different eyes of the same individual, while no two successive fixations of the same point by the same eye are ever exactly the same. In general, however, his diagrams indicate an area of wandering regard corresponding to somewhat less than one degree of eye movement.

McAllister's results obviously justify Judd's use of the term *fixation*. On the other hand, in spite of the fact of continuous movement, it seems worth while to distinguish between those purposive eye movements that condition clearer vision, and the minute non-purposive oscillations that occur during moments of comparative quiet. The justification for the discrimination is the difference of function. But it should be remembered that whenever we speak either of ocular or of visual '*fixation*' we mean the moments of restricted mobility which have heretofore commonly been called by that name.

A simple and fairly satisfactory class demonstration that the ocular fixation movements (fixation pseudonystagmus) really correspond to unconscious and involuntary movements of the point of regard may be produced by projecting a good negative after-image of an illuminated wedge on a sheet of

[1] Monograph Supplements of the PSYCHOLOGICAL REVIEW, Vol. VII., pp. 17–53.

cross-section paper. Even the utmost effort to 'fixate' some point on the cross-section paper, will not prevent a fatal inability to keep the after-image quiet. The experiment is usually surprising enough to the student, since these rapid fixation movements usually give absolutely no introspective evidence of their existence, although they persist in spite of every effort to hold the eyes rigid.

Unfortunately, two factors coöperate to partially invalidate this after-image method as a means of investigating the normal extent of the fixation movements. First, the obvious wandering of the after-image itself gives immediate warning of the lapsed fixation. The lapse is more quickly corrected, and consequently the amplitude of the fixation movements is much less than that discovered by objective registration. On the other hand, a more or less strongly marked tendency to ignore the nominal fixation mark and to attempt to fixate some part of the after-image leads to occasional breaks in the rhythmic oscillation of the after-image about the fixation mark. Without warning, and without intent, the after-image suddenly darts off in the direction of its center of gravity, and the true fixation is renewed only after an appreciable interval. These two invalidating factors are apparently opposed in direction, but they are not mutually inhibitory, since the second factor is intermittent and always occurs in selected directions.

In my own case the diameter of the area of wandering fixation as measured by the above inaccurate method is about 10′. The amplitude of the occasional movement in the direction of the center of the after-image is more variable but in general it lies within a circle whose diameter is half a degree of eye movement. Both forms of fixation movements, but more especially the latter, seem to be increased by visual fatigue. The former variation, on the other hand, is much more pronounced after bodily exertion. This fact together with a curious regularity in some of my records lead to the conjecture that one factor in the rhythmic fixation movements is due to vasomotor changes. The main theoretical value of the after-image demonstration is the conclusive evidence which it furnishes that the rhythmic fixation movements involve actual

movements of the point of regard and are not merely misinterpreted head movements.

Since in the strict sense of the word there is no absolute fixation it follows that there is no true fixation point. Without disturbing the time honored equivalence of the terms 'fixation point' and 'point of regard,' I would propose the term *fixation field* to include that part of the field of regard which is traversed by the point of regard in the fixation movements. The new term is analogous to the term field of regard. It immediately suggests fixation movements, and is not otherwise in common use.

§ 2. *The Physical Origin of Fixation Movements.*

The question as to the causes of the fixation movements has never received systematic investigation, as far as I am aware. I am not sure it deserves systematic investigation, but there are some factors at least that are significant. Among the general limitations to strict ocular fixation doubtless the most obvious is the nature of muscular action. Opposed muscle tensions are never absolutely balanced for any length of time in normal life. No part of the body is rigidly motionless, and the movements of the more mobile members need no refined demonstration. The involuntary movements of the head and hand bear a close family resemblance to ocular fixation movements. But in addition to the more or less intermittent waves of excitement which the eye muscles doubtless share with all other contractile tissue, it is obvious that any lack of rigidity of the head and trunk muscles must result in passive displacement of the eyes and a consequent disturbance of visual fixation unless the movements of the head and trunk are accurately compensated by delicately adjusted coördinate eye movements.

Involuntary movements of the head do not lend themselves readily to mechanical registration. Direct tracings must lose many of the more minute movements. And even a system of levers is hardly sufficient to ensure an adequate account of the complex variations in the axis of rotation. A fairly good demonstration of the nature and amplitude of those especial head movements that would most certainly produce passive

eye movements may be made with a pair of smoked glasses. It is a fact, familiar enough to all those who have the misfortune to wear glasses, that the reflections of objects behind the observer may on occasion superpose themselves on objects seen through the glasses. But a movement of the head causes a movement of the reflection quite different from the apparent movement of the object seen directly in front, through the glass. Smoked glass increases the relative brilliancy of the reflection. If one glass in a pair of smoked spectacles is bent slightly away from the eye, the reflection of an artificial window may be made to cover a piece of cross-section paper seen through the glasses. Lateral displacement of the head will be shown by the apparent movement of the artificial window reflection over the cross-section paper. And the amount of the apparent displacement can be read off immediately in divisions of the paper. Somewhat less satisfactorily direct observation of the head movements may be made by attaching a light rod to a Helmholtz mouthpiece. The apparent movement of the end of the rod across the cross-section paper may be reduced to tangents of the angle of head movement. But the weight of the long pointer introduces a foreign element; and the mouth bit arrangement precluded the use of the device for testing the reliability of various head rests. Moreover, the former method has the advantage of giving readings double the amplitude of the actual angular displacement of the head.

Unfortunately both the above methods are subject to much the same criticism that invalidated the after-image test of fixation movements, viz., the data, on which the value of the tests depends, furnish an unusually delicate indication that the head has moved and constitute an unusually accurate motive for corrective movement.

Notwithstanding these unusual motives for the limitation of head movements they are by no means eliminated. On the contrary the practically continuous movements show at least three interacting factors.

(*a*) The most regular movements consist of a succession of incisive, rhythmical jerks, of nearly constant amplitude under

similar conditions. In character and frequency these movements immediately qualified as pulse movements. They show the typically sharp pulse stroke and the slower, more irregular recovery. They appear under the most diverse positions of the head, but the direction of the initial stroke and the consequent recovery change notably under different conditions. When my head is approximately at its primary position the greatest elongation of the main stroke is vertical. When my head is turned 50° or 60° to one side the main stroke is oblique.

(*b*) Beside the rhythmic pulse movements there are less frequent, more irregular disturbances coincident with breathing.

(*c*) Finally, even when the respiration movements are temporarily inhibited, there are irregular drifting movements interacting with the pulse movements.

The amplitude of the combined movements varies with the general excitement, and with the amount and character of support furnished the trunk and head. When sitting quietly on a stool without back rest the rhythmic pulse movements fell within a circle whose total diameter was approximately 10′ of head movement. The occasional extreme movements, coincident with respiration, under the same circumstances, fell within a circle whose diameter was 20′ of head movement.

No support for the trunk either at the back or front modified to any appreciable degree the amplitude of the rhythmic pulse movements (type *a*) ; but merely resting the head on the hands reduced all the movements somewhat. When the head was pressed firmly against a rigid side support, the total movement in five minutes including half a minute of eye closure, was 10′ in a vertical direction, and 5′ in the horizontal. The rhythmic pulse movement was very small, approximately 1′. Using the form of head rest that I ordinarily employ to hold the head for photographing the eye movements, reduces the rhythmic movements of the reflection to a barely perceptible tremor the amplitude of which could not readily be estimated but seemed to reduce to perhaps 15″–30″. Actual photographs of these tremors, Plate I., Fig. 2, reduce to 111/291 of one minute of head movement. Breathing movements on the other hand and the irregular drifts whose source is legion,

were never even approximately eliminated. With care my head rest will keep the head within a total movement of 5′ in five minutes. In actual practice, however, without the additional control of the moving reflection, I find that my photographs indicate a total head movement of somewhat greater amplitude.

In total amount, if not in kind, the head movements would account for all the fixation movements of the point of regard, if the eye were immovable in its orbit. But they would account for more than actually exists. Furthermore, while the amplitude of the head movements steadily decreases with increasing stability of the head rest, the amplitude of the fixation movements of the point of regard is relatively independent of the amplitude of the head movements. Indeed I have repeatedly noted an increase in the oscillations of the point of regard concurrent with the fixing of the head firmly in the head rest. This, however, was an evanescent phenomenon which did not lend itself readily to accurate determination. The important fact remains that when the pulse movements of the head are reduced to a mere quiver the amplitude of the fixation movements of the point of regard remains essentially unchanged. Equally significant, on the other hand, is the evidence that movements of the head and eyes do not directly correspond. See Plate I., Fig. 2.

Besides the fixation movements resulting from pulse and respiration, the eyes suffer passive displacement with every movement of the head and body. For a great variety of such somatic movements there exist coördinate compensatory eye movements through which, without reaction interval, a surprisingly accurate fixation is maintained. So regular and persistent are these coördinate compensatory eye movements[1] that, except when the eyes are strongly converged, it is impossible for me to move my head with the eyes fixed in their sockets, *i. e.*, so that the eyes move directly with the head. That the normal coördinate compensatory eye movements are really coördinate with the head movements and are not reactions may be shown by a simple experiment entirely without the aid of registering

[1] *Am. Jr. of Physiology,* Vol. VIII., p. 322, fol.

apparatus. If three or four lines of print are placed at a comfortable reading distance before a subject, it will be found that head movements of moderate amplitude and velocity do not seriously interfere with the process of reading, *i. e.*, notwithstanding the head movements the fixation pauses of reading are sufficiently well maintained to prevent blurring of the printed matter. If, on the other hand, the same lines of print are attached to a light rod and the latter be held firmly in the mouth so that the whole moves with the head, it will be found that the process of reading becomes difficult and even impossible as the head moves back and forth. Now it is obvious that if the compensatory eye movements were reactions to the apparent movement of the field of view in the first case then there would be some considerable blurring of the print before the eye movement got under way, which is contrary to fact. Conversely, the absence of all apparent movement of the object of regard, with respect to the head, would produce no stimulus for reactive eye movements, but the coördinate compensatory eye movements actually occur just the same even though they seriously disturb or even entirely inhibit the process of reading, by blurring the field of vision.

The same results are obtained if a row of black dots is used instead of letters. Indeed the latter arrangement has some advantages over the letters since the character of the visual disturbance is even more apparent. The experiment conclusively shows that during normal head movements the eyes do not remain fixed in their sockets. It shows furthermore that the eye movements which compensate for normal head movements are not reactions to the apparent movement of the field of view, but are in fact, as we maintained, coördinate to the head movement, and have their origin in the same impulse that moves the head. In one way at least these coördinate compensatory eye movements are of unique theoretical interest, since in them more completely than in any other motor experience with which I am acquainted, the eye movements and the movements of the head and body are united into a thoroughly organized motor system. When one realizes that coördinate compensatory eye movements occur with every step

in walking, with every movement of the legs and arms, as well as with every movement of the head, it is obvious that we have come upon a coördinating mechanism that is thoroughly capable of explaining the intimate correspondence between tactual and visual space. These facts will serve us later when we discuss the spatial organization of the retina in Chapter IV.

There are many bodily movements, on the other hand, that have no coördinate compensatory eye movements. Passive movements of the head obviously could have no true coördinate compensatory eye movements, and as a matter of fact they do not have them. If, when the subject is seated in a revolving chair while the reading matter is attached to an independent support, the chair is rapidly rocked back and forth, producing rapid passive movements of the body and head, reading will be grossly disturbed. The experiment is best if irregular oscillations are made cr. 4–5 per sec. If on the other hand the printed matter is attached to the chair so as to move with the head in its passive movements there is little or no disturbance of the reading process. The results of the experiments with the passive movements of the head are thus seen to be directly the reverse of those with voluntary movements. The spasmodic eye movements that tend to appear during passive movement of the head even when the eyes are closed, show a true reaction interval and may be regarded as reactions to some non-visual perception of movement. Furthermore, voluntary movements of the trunk from the hips, in which movement the head is held rigidly in line with the trunk, show no coördinate compensatory eye movements. Finally, I can detect no true coördinate compensatory eye movements for the indefinite, non-rhythmic, and irregular head movements.

In view of all these facts I venture the generalization that, notwithstanding the delicate coördination between some head and body movements and the eye movements, there still exist in the adult, probably in the pulse movements and surely in the irregular head and body movements, entirely normal and continuous physical disturbances of visual fixation. I have purposely left out of consideration all abnormal disturbances such as occur in dizziness, nystagmus, etc.

§ 3. *Visual Motives for Fixation Movements.*

Besides the physical sources of fixation movements there seems to be some evidence that there are visual motives, though I confess that for one of these the evidence does not seem altogether conclusive. Our problem must not be confused with the question whether or not there are visual motives to follow a moving object. Of that there can be no doubt. Neither should it be confused with the question as to the natural limitations of the visual control of fixation. That will call for special discussion later. Our present problem concerns the fixation of a motionless object; and the question is whether there are or may be visual motives to make such fixation relatively unstable. I believe such a motive exists in retinal fatigue, and the involuntary shifting of the functional center of the retina. It is now a well-known fact that a single point on a plain field is the worst possible fixation mark, and permits the widest possible fixation movements. That it actually induces fixation movements would be difficult to demonstrate. But I believe it may and probably does on the following grounds: There is little doubt that one of the most important signs of satisfactory fixation is the clearness and sharpness of a visual impression. Now it is evident that if by some perfect adjustment a point of light exactly 1′ in diameter should fall continuously on a single retinal element at the fovea, the development of fatigue in that retinal element would bring about a gradual fading of the impression until it disappeared. At every moment of our hypothetical case, after fatigue had set in, it appears that the impression would be clearer and brighter if the stimulus should pass from the original fatigued element to some neighboring non-fatigued retinal area. But the lessened sensitiveness of fatigued retinal elements is the same whether the object is a visual point or a complex of several points. Under ordinary circumstances then clearest vision would be subserved not by such an absolute fixation as should keep the same stimulus constantly on the same retinal elements, but by a more or less rapid substitution of fresh for fatigued retinal elements. A similar function is subserved by winking. Moreover, the natural observation of an object involves more

or less rapid alternation of fixation pauses separated by eye movements, as we look successively at its various parts. If for any reason it became necessary to continue the fixation of a single point indefinitely it certainly does not seem improbable that visual fatigue would operate to produce a shifting of the image from fatigued to less fatigued portions of the fovea. Certainly some of the movements of continuous fixation records are more easily explained by that hypothesis than by any other. See Plate I., Fig. 2. And there is abundant evidence for a shift in the functional center in dark adaptation. Even if we hesitate to admit visual fatigue as a positive motive to produce fixation movements, it must very obviously serve negatively as a limitation of the visual motives which tend to check such fixation movements.

A second visual motive for fixation movements is clearly demonstrable in binocular vision in the correction of inadequate binocular coördination. Movements of convergence and divergence complicate even the simplest changes in the position of the line of regard. In some cases, as Judd has shown in the only accurate discussion of convergence movements ever published,[1] these movements of convergence are coördinate with the rapid movements in connection with which they appear, and occur without adequate reaction time. In other cases as in some photographs which it is not necessary to discuss further in this place, the slow corrective convergence movements may occupy the entire period of nominal fixation, and, at the end at least, they are doubtless reactions to unsatisfactory visual conditions.

It is with some hesitation that I speak of movements of convergence as fixation movements at all. It may be objected that they constitute rather a part of the precondition of fixation than fixation itself. But there is no doubt that they may occur during moments of relatively clear vision, and they thereby differentiate themselves from those eye movements in which there is no new adequate stimulation of the retina, whose only function is to secure more favorable conditions for viewing the new object of interest.

[1] 'Records of Convergence and Divergence,' The PSYCHOLOGICAL REVIEW Monograph Supplements, Vol. VIII., pp. 370, fol.

§ 4. *The Inhibition of Fixation Movements.*

There is no natural guarantee for fixation of any sort. There is no pre-established harmony between any object in the external world and any special part of the human retina. On the contrary every new act of fixation is in consequence of some discreet stimulus to eye movement; and in view of the continuous displacements of the eyes by head and trunk movements, every act of fixation is maintained only by a more or less continuous process of readjustment. There are only two apparent means for stimulating the eye muscles to preserve fixation or to reëstablish a lapsed fixation. One of these means is the physiological coördination which we have seen actually operates only within certain well-defined limits. The other is the direct peripheral stimulus involved in the lapsed fixation itself. The former presupposes prevision of the direction and extent of the head movement and would be helpless to correct either a chance inaccuracy of coördination, or an irregular displacement for which there were no corresponding coördinate compensatory eye movements. In all such cases the reëstablishment of the fixation must depend on some sensory stimulus involved in the lapsed fixation itself. Kinæsthetic data would be inadequate for slight lapses even if they existed. But there is no evidence that kinæsthetic data ever provide the stimulus for reëstablishing even the grossest lapses of fixation. And, in view of the presence of purely visual data corresponding exactly with the only possible motive for ever reëstablishing any fixation, there seems to be little ground, either biological or psychological, for the development and refinement of kinæsthetic control. The visual control shown in our after-image experiment was an artificial affair. As we have already noted, it was complicated by the presence of the well-defined after-image. This is seldom duplicated in natural vision, but it is closely approximated during the fixation of sharp boundaries between contrasting surfaces, when a fringe after-image, often confused with irradiation, may function as a delicate datum for lapsed fixation. It is directly congruent with this general principle that sharp lines tend to restrict fixation movements across them, and conversely, that

the elimination of sharp boundaries as in Myer's contrast experiment increases the tendency to fixation movements. Ordinarily the visual control of fixation lies between these extremes and, as photographic records show, ordinary visual control permits considerably wider fixation movements than our after-image experiment discovered.

Even under the most favorable conditions of visual control the fixation movements are altogether disproportionate to the sensitiveness of the retina to local difference or to the movement of objects across the field of view. At least one element which tends to produce this disproportion is the relatively long ocular reaction time. Obviously no visual clue to a lapsed fixation could act except as a stimulus to an ocular reaction. But since the physical displacement of the line of regard operates not only up to the point where it constitutes a stimulus for corrective eye movement, but is also operative more or less irregularly during the interval between the stimulus and the reactive eye movement; it must follow that, even if the reactive motor impulse were exactly sufficient to correct the lapse at the moment of stimulation, it would effect an exact correction only occasionally and accidentally. Unless the intervening eye movements were sufficiently regular to permit some prevision of their probable extent and direction, there could be no absolute correction unless they accidentally happened to exactly neutralize each other. Apparently some of the movements admit prevision. This appears to be the case with respect to the pulse movements. After the first few pulsations under any given circumstances the head pulses seem to be automatically corrected by short-lived ocular habits. Plate I., Fig. 2. The insufficiency of the correction of the irregular head movements would be directly proportionate, other factors remaining equal, to the length of the ocular reaction time.

§ 5. *The Reaction Time of the Eye.*

The available data relative to the reaction time of the eye to a visual stimulus is very limited. Following a method devised by Professor Erdmann and myself in 1899, I published a series of observations in 1903 giving my own average reaction

time to a peripheral retinal stimulus as 165 σ. Huey confirmed the generalization that the reaction time of the eye is relatively long. The development of photographic registration seemed to give opportunity for relatively exact objective measurements. Two series of photographic records confirmed in general the long ocular reaction time, but both series contained an apparently ineradicable source of error, rendering the results comparatively valueless. The difficulty in these records was the fact that each record consisted of three separate lines on the photographic plate, viz., a time line and a signal line besides the corneal reflection line. Since these lines necessarily differed more or less in their density, there was an obvious source of error in reading the plate, which error was only slightly less than the total width of the slit through which the recording beams fell on the photographic plate. It soon became evident that the number of lines must be reduced and I had already arranged a tuning-fork interruption for the beam of sunlight when Holt reported satisfactory results with the alternating arc light. That solved my difficulty and made it possible to record in a single line, the time, the beginning of the stimulation, and the reaction.

The general arrangement of the apparatus is given in the line drawing Fig. 1. *C.* is an enlarging camera approximately

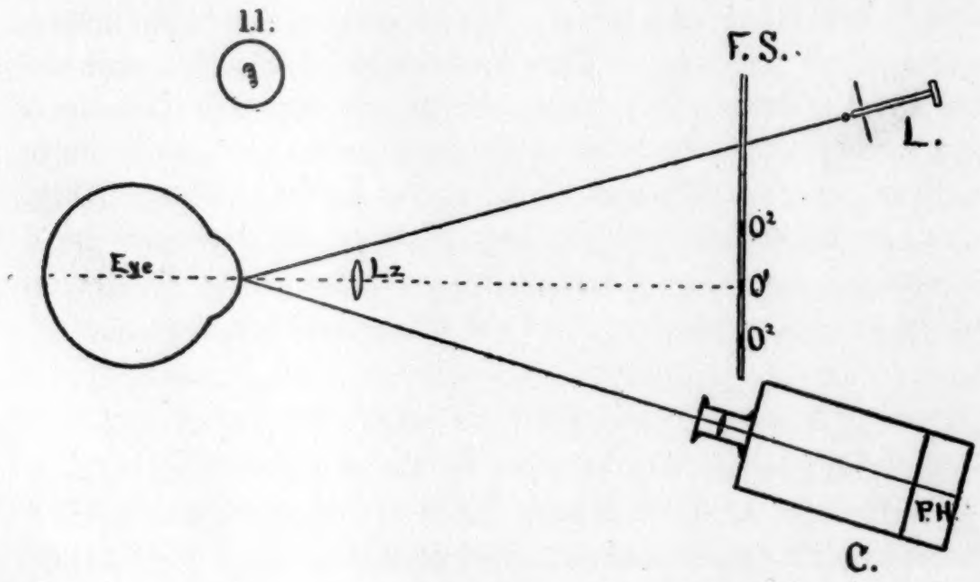

FIG. 1.

4 ft. long. *P.H.* is the box holder containing the mechanism for the regular movement of the plate. *L.* is the alternating current arc light, enclosed in an asbestos-lined box, with a small aperture to confine the light to the reacting eye; this aperture is covered with from one to three thicknesses of pot blue glass to stop out all but the actinic rays. *F.S.* is a falling screen exposure apparatus in which the falling screen is so placed with reference to the arc light and the object that at the same moment that the object is exposed the recording beam of light is flashed to the cornea. O^1 is the preëxposure fixation mark. O^2 is the object stimulus to reaction.

A photographic reproduction of the apparatus in position is given in Plate III., opposite page 93.

The apparatus functions as follows: The observer sits comfortably in front of the camera and fixates an arbitrary mark O^1 on the black shutter of the falling screen exposure apparatus. The whole is brightly illuminated by incandescent electric light *l.l.* Full correction for any visual error is made by placing the proper correcting lens in a standard at *Lz.* The head is held fairly rigid (page 94) by the heavy head frame. After an appropriate signal, when the sensitive plate was allowed to fall slowly, the black shutter holding the preëxposure fixation mark was released and withdrawn by a spring. By a movement of less than half an inch the falling screen simultaneously exposes the object word at O^2 and opens a passage for the recording beam of light. The beginning of the exposure, therefore, is simultaneous with the beginning of the record on the plate. Since the coincidence of the beginning of both the exposure and the record is mechanically conditioned, while the beam of light has neither latent period nor inertia the conditions for recording the beginning of the stimulus are almost ideal. The ocular reaction to the peripheral stimulation is recorded by the beginning of obliquity in the photographic line. Here again the registration is accomplished without latent time or any other constant error. The form of exposure of the word stimulus is not altogether satisfactory, involving as it does, the uncovering of the word by a falling screen. The fault was difficult to obviate. It was minimized

PLATE I.

Text to Plate I.

FIG. 1. Line drawing from the photographic record of the horizontal displacement of the corneal reflection during the supposed fixation of a dimly lighted black dot on a white field. Both head movements and eye movements are included in the record. The pulse movements are clearly visible, and are indicated by dots.

FIG. 2. Line drawing of an enlarged record of horizontal head and eye movements during the supposed fixation of a well-lighted black dot on a white field. The head line at the left shows the rhythmic pulse movements and the slower drifting movements. The eye line at the right shows how in prolonged fixation the pulse movements of the head may be completely compensated.

FIG. 3 is an enlarged record of a photographically registered reaction to a peripherally exposed word. The beginning of the record, reading up, is the beginning of the stimulus. The first break in the line is an artificial break to indicate which way the plate is to be read. The beginning of the eye reaction to fixate the exposed word is shown by the oblique displacement of the record, *C*. The second smaller displacement, *D*, is a corrective movement, showing that the first movement of the eye failed to carry it far enough. The beginning of the speech reaction is shown by the ending of the record, *E*.

FIG. 4 is an enlarged record of the ocular reaction to a moving pendulum. The beginning of the record is coincident with the beginning of the movement of the pendulum. The first lateral displacement in the record shows the first indication of reactive pursuit movement. Successive lateral displacements show the typical corrective movements found at the beginning of all pendulum pursuit movements. It will be noticed that an approximation of the pursuit begins immediately after the first rapid reaction movement.

FIG. 5 is an enlarged record of pendular pursuit movements, after the initial irregularities have been overcome and the eye has settled down to the true pursuit. It will be noticed that even here there are slight corrective movements, occurring chiefly at the center of the arc of oscillation.

FIG. 6 is an enlarged record of the attempted pursuit of the pendulum, when for about one quarter of the arc of swing the pendulum passed behind an opaque screen. The gross disturbance of the pursuit could not be obviated by any amount of effort.

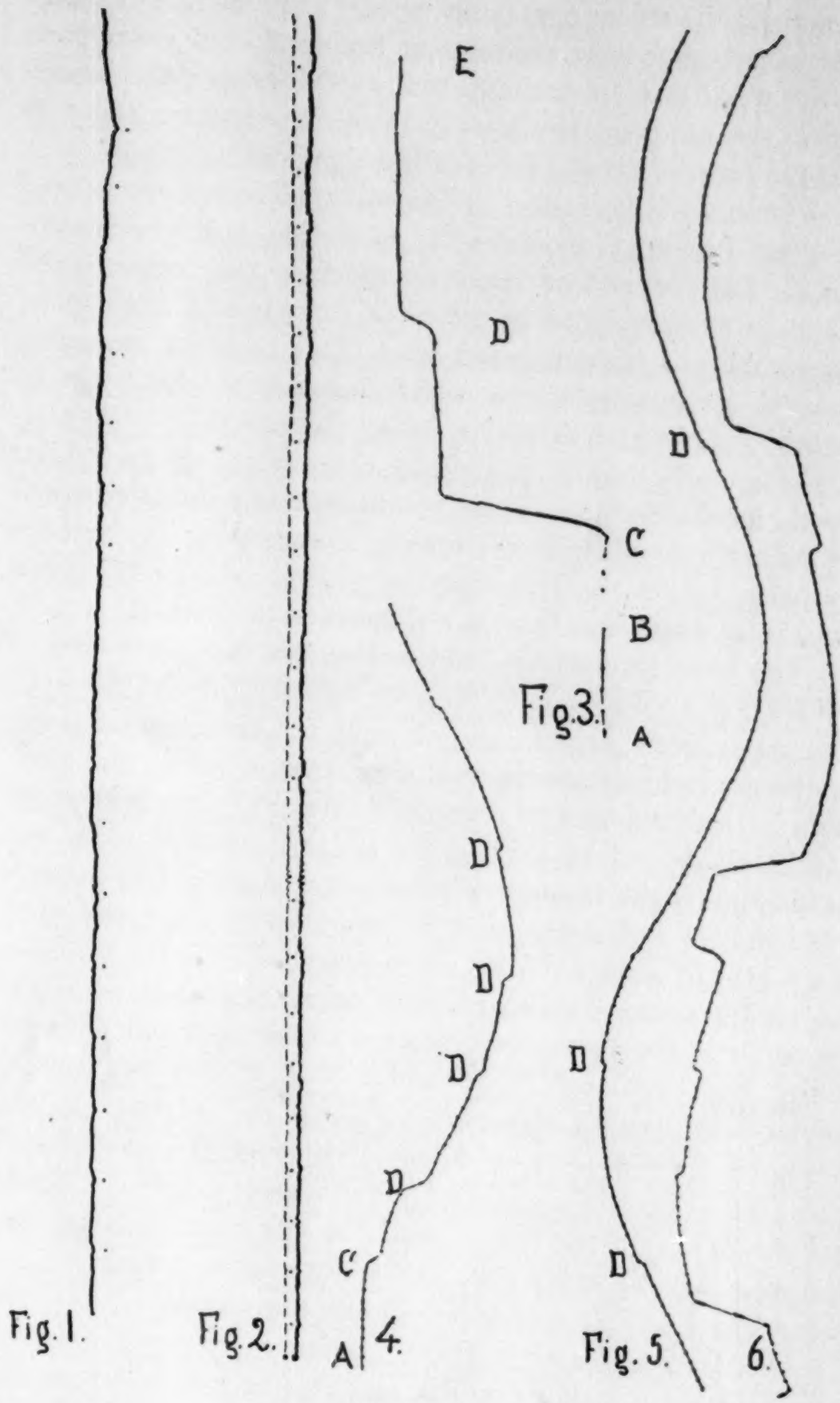

PLATE I.

by making the screen operate as rapidly as possible, so that the difference between the exposure of the upper and lower parts of the word was approximately 1 σ. Moreover, the investigation was not primarily a study of the process of reading, so that the form of exposure was of less significance.

The end of the record in the enlarged record reproduced on Plate I., Fig. 3, indicates the conclusion of a speech reaction. The first puff of breath in speaking the exposed word broke an electric circuit by means of a modified Cattell sound key combined with our mouth bit. This break in the circuit released an opaque screen which interrupted the beam of recording light and therewith ended the record.

In figures 3 to 6 the punctiform character of the record line should be observable. These points represent the alternations of the arc light and give the time. A continuous line connects the dots due to the fact that the globule of molten carbon remains at white heat from one alternation to another.

The photochronograph above described has some obvious advantages over all other forms of recording device for reaction experiments in general. Except for the constant drone of the arc light its operation is altogether noiseless, and there is no intrinsic reason why even the drone of the arc light might not be eliminated by placing the lamp in another room and passing the light through a transparent door. The records are indelible and easily read by projection. Since one plate 5 x 7 in. will serve from 10 to 20 reactions, it is not prohibitively expensive. The main limitation to its general use is the variation in the alternating current. This is a real difficulty when great accuracy is demanded, but it may be entirely obviated by using a direct-current lamp and interrupting the beam by a tuning fork of known frequency. I have tested this arrangement in a series of measurements and find it fairly satisfactory. In the tests I made of our alternating current it proved reliable to less than half an alternation a second. Its mean value was 61.6 complete phases per second.

One further factor in these reaction experiments deserves consideration. It is a familiar fact that an intense peripheral light constitutes a more or less serious disturbance of all the

retinal processes. Unfortunately, however, a highly actinic beam of light is indispensable for photographic registration, and the problem reduced itself to producing a highly actinic beam of light with the least physiological effect. This was accomplished by stopping down the arc light by plates of pot blue glass. This blue glass only slightly reduces the effect of the light on the plate while it reduces the visible effect of the arc light from the annoying glare of the naked arc, which dimmed the whole field of view, to a comparatively insignificant appearing glow, which scarcely had sufficient physiological intensity to provide a perceptible after-image for experiments described in the Appendix. In respect to its physiological effect as well as in general convenience I found the arc light more satisfactory than sunlight.

The result of the simple eye reactions to words of four letters is given below in Table I. No records are omitted.

TABLE I.

A. When the center of the 41′ word stimulus was 45′ or 1° 30′ either to the left or to the right of the line of primary fixation.

No.	Average.	Lowest.	Highest.	Mean Variation.
14	166.6 σ	138 σ	186.6 σ	13.6 σ

B. When word stimulus was 45′ or 1° 30′ to the right only.

No.	Average.	Lowest.	Highest.	Mean Variation.
17	151 σ	130 σ	163 σ	9.9 σ

C. When word stimulus was 4° 30′ or 6° to the right only.

No.	Average.	Lowest.	Highest.	Mean Variation.
9	181 σ	146 σ	252 σ	19.1 σ

Notwithstanding the general reliability of these results, it would obviously be inadmissible to apply them directly to the correction of a lapsed fixation. They are, to be sure, eye reactions, but they are reactions to a complex new peripheral stimulation, and involve a change of attention from the preëxposure object to the exposed word. It is certainly not *a priori* impossible that reactions to a lapsed fixation would be shorter. In view of these considerations I arranged a control experiment which approximated rather closely the conditions of a lapsed fixation. While the subject fixated a sharp point of light on the bob of a second pendulum the latter was set in motion by

breaking an electric circuit. The same circuit also held a shutter over the beam of light, so that coincidentally with the beginning of the movement of the pendulum the record began. Plate I., Fig. 4, reproduces a typical record of these control experiments. Unfortunately they are too few to constitute a proper demonstration, but it is significant that the one reproduced shows a reaction time of over 180 σ. Records of the pendulum reactions for other eyes than my own are more numerous but I have no eye reactions to the moving pendulum less than the minimum reaction to an exposed word, *i. e.*, 130 σ.

In all eye reactions there is a certain amount of pre-reaction eye movements. In view of the prevalence of fixation movements, even under the most favorable circumstances, these pre-reaction movements are inevitable. They constitute a beautiful example of the unstable equilibrium discussed by Judd in the first volume of the Yale Studies, and in this case at least some of the factors producing the disturbance are known. But the pre-reaction eye movements constitute a serious menace to the accurate reading of the reaction records. In the present instance, moreover, there is apt to be an additional complication in the increased tendency to head movement that accompanies reaction to the pendulum movements. This is particularly noticeable in untrained subjects. The apparent paradox seems to obtain that the head reaction is considerably more rapid than the eye reaction.

As far as the visual factors are concerned, however, the pendulum control reaction is satisfactory enough. The effort was consistent throughout to maintain the fixation of a well-defined point. The lapsing of that fixation, in this case by a movement of the pendulum, involved no wandering of the attention. The reacting eye movement was directly demanded by the original effort to maintain fixation. In this case, as in simple fixation, it occurred in response to the necessity to reëstablish a lapsed fixation.

A study of the records of fixation movements shows some rapid movements occurring in succession without the intervention of full reaction intervals. At first sight this might appear to indicate some form of ocular reaction more rapid

than that which we have succeeded in obtaining experimentally. Naturally this is not absolutely impossible. But, on the other hand, there is abundant evidence, which we shall regard in detail in the chapter on the complication of the visual process, that successive stimuli may initiate reactive movements of the eyes, and that the latter may appear successively in approximately the same tact as the stimuli. Moreover the reactive correction of the slow drifts is not more frequent than the reaction experiments would lead one to expect.

§ 6. *The Fixation Object.*

In view of the persistent irregularities of normal fixation, in view of the numerous physiological and psychological motives for fixation movements, and finally, in view of the slowness and general inadequacy of the correction of lapsed fixations, the traditional assumption of an identical anatomical and functional center of the retina, to which all visual processes are referred or referable, is untenable. A functional center in the sense of a retinal *point* which normally corresponds with a *point* of interest does not exist. Even if for the sake of the discussion we could admit that primitive vision involved the sharp differentiation between a central element and all other parts of the retina, there are obvious psychophysical motives which would tend to break down the distinction in the interests of clear vision. The continuous rhythmic movements of pulse and respiration would occasion continuous illusions of motion, which the increased amplitude of movement during the long ocular reaction intervals would further exaggerate. Furthermore, the perpetual inaccuracy of what subjectively passes for fixation and refixation, must tend to break down the too accurate differentiation. But there is no evidence for such a con-natal preadjustment of vision. Indeed all the evidence is directly opposed to it. And there is certainly no adequate motive for its development.

On the contrary, and in addition to the grounds already exploited, there is experimental evidence that for any given object of regard several retinal positions are practically equivalent.

TABLE II.

occasional	gratifying
thoughts	statements
judicious	liberty
practicable	popular
experts	accuracy
the articles	experts
paragraphs	extent
degree	extended
to the notes	practicable
series	arti-
h	conscience
e	thoughts
needful	paragraphs
week after week	judicious
pages that follow	x

The experimental evidence on which I venture this assertion is a series of experiments to determine the natural fixation points of words and groups of words. In recognition of the extreme difficulty of referring any definite point of an objective record, by whatever method it is obtained, to any definite point in the field of regard, I used the only subjective method which is absolutely reliable and trustworthy for the determination of isolated fixation points, viz., the after-image method. A good after-image was obtained by the continuous fixation of

the apex of a brightly illuminated wedge. Then sitting at a convenient reading distance from an upright standard the subject uncovered a word held at about fifteen degrees to one side of the wedge. The word configuration was clearly seen while the word was still illegible. Direct fixation of the word for a moment gave the word percept while directly under or about some part of the word a clear after-image of the wedge indicated the point at which the point of regard had actually rested.

The result of the fixation experiment is given in detail in Table II. The same words and word groups were exposed in three different series. A line above the word indicates the first series. A dotted line indicates the second. A line below the word indicates the third. Any combination of these marks indicates coincidence of the fixation in two series. A large number of control experiments by myself and students gave similar results.

These results are in direct agreement with the wide variations in the successive fixations of the same object that have been noted by Stratton, Dearborn, McAllister, and Judd. These variations may not be regarded as accidental errors symmetrically grouped around a theoretically perfect fixation. They must be regarded as a chance distribution over relatively indifferent retinal elements. Each case is a perfect fixation, not in the sense that some objective point falls in each case on the anatomical center of the retina, but in the sense that the object of interest is brought to a retinal area of clear vision. Anything more would be useless exactitude and nature is seldom uselessly exact. The psychophysiological dogma that there is a tendency to transfer every peripheral stimulus to a fixed punctiform retinal center is a myth. There is no punctiform functional center of the retina. The functional center varies in individual cases. It may be a larger or a smaller area according to the character of the object of regard and the corresponding extent of the area of clear vision. Only artificially is the peripheral object of regard a punctiform object. Usually it occupies an area of appreciable extent, and the object of fixation isn't a point but an area. The psychology of reading has had a long fight against the preconception that

the object of regard must fall on the fovea, and I think there is still a tendency to give undue prominence to the location of the center of any object of regard. The location of the 'point of regard' within an object of regard is valuable, it seems to me, rather because it gives some indication of the nature and extent of the object of regard than because it locates the position of a schematic point of fixation.

As a matter of fact, fixation is confined to no particular part of the retina. The possibility of extra-foveal fixation is abundantly proven for specific circumstances. Aside from the unusual cases of the development of artificial fixation areas, such as the case reported by Storch,[1] normal extra-foveal fixation is a familiar fact of dark adaptation. Moreover the greater part of any fixation object is extra-foveal. To be more specific: if this page is held 12″ from the eye, the fovea will be covered by the enclosed part of an *a* and the entire macula will be covered by three or four letters. Now no one would venture to assert that the significant part of the stimulation in any reading pause was given in an area smaller than the enclosed part of an *a*. Practically all the significant stimulation of the retina in any reading pause then is extra-foveal. To insist that a fraction of a letter is fixated at any reading pause would mean only that an imaginary line called the line of regard passes through that letter, or rather it means that at any given instant the line of regard passes through some point of that letter. That some particular point of the letter determined the extent of the prefixation movement would be as absurd as to insist that since the line of regard passed through a point of the letter consequently that point constituted the real object of regard and the immediate object of perception.

Not only does the greater part of any natural object of regard lie outside the fovea, but there is direct experimental evidence that extra-foveal vision may be the determining factor in maintaining fixation.

If a narrow strip of cardboard be interposed between one eye and the object of regard situated some feet distant on the opposite wall of a room, it may be made to cover several de-

[1] *Zeit f. Psy. and Phys. d. Sinnesorgane,* XXVI., 201–226.

grees of the field of view without preventing binocular adjustments sufficient to produce a fusion of the congruent parts of the two fields of view. This is the more remarkable in my own case since I have a slight hyperexophoria. Whatever it is that determines the position of the eye before which the interference is placed, it must be something aside from both muscular balance and foveal stimulation, and that something must originate in the influence of extra-foveal corresponding points.

A still more frappant demonstration of the extra-foveal determination of the position of the eye is furnished by a chance observation. While I was studying the rhythmic disturbances in fixation by means of the after-image method, I tried a number of backgrounds to see if they might influence the accuracy of fixation. Among other expedients, I projected the after-image on a background of cross-section paper seen through an aperture in a similar screen which was more heavily marked and suspended on a pendulum. On setting the pendulum lightly in motion I noticed wider oscillations of the after-image than before. These wider oscillations persistently followed the tact of the pendulum and covered substantially the same amplitude. Notwithstanding persistent effort to fixate a point of the background that was never covered by the swinging screen, I found it absolutely impossible to avoid following in some measure the movements of the pendulum screen. For the best results the pendulum screen should be heavily marked by some prominent figures and the aperture should not be over ten degrees. I used a second pendulum. It seemed and still seems a striking proof of the influence of extra-foveal and extra-macula determinants of fixation. Doubtless the same principle underlies the waterfall illusion.

Somewhat more exact data are obtained by the records of pendular pursuit movements. If the eye attempts to follow an object of regard which is attached to a moving pendulum, after a relatively short preparation period extending approximately through one complete oscillation of the pendulum, the eye catches the tact of the pendulum and the pursuit movement is broken by relatively few rapid corrective movements. A record of the pendular pursuit movement in its first stage is

given on page 17, Plate I., Fig. 4. The advanced stage is given in the fifth figure of the same plate. If now a screen be placed between the eye and the pendulum so as to shut out from view the middle of each oscillation, no amount of effort can force the eyes to maintain the tact of the pendulum. Plate I., Fig. 6. If on the other hand the screen is so placed as to hide only a part of the bob including the fixation mark, the interruptions again become insignificant.

In view of all the evidence I venture the following generalizations:

The hypothesis of an intra-foveal fixation point, differentiated from all other retinal points as the point of clearest vision, to which all other retinal points are related by a system of motor impulses, which are calculated to bring every point of interest to the fixation point, may perhaps be schematically helpful but it does not represent the facts.

Every field of vision on the other hand embraces a clearer and more distinct central area whose extent varies with the character of the object of regard. But the area of clear vision merges into a less clear and distinct peripheral area without sharp boundaries.

It seems to me that the psychological value of determining the positions of the line of regard in the succession of fixations which constitute the moments of significant stimulation in normal vision, is not to determine the position of the scientific abstraction called the point of regard but to determine the center of the more or less extensive area of clear vision.

It seems to me, farther, that the real problems of psychological optics are not connected with the mythical fixation point so much as with the interrelation between the areas of more and less distinct vision.

Chapter II. Adequate Fixation.

§ 1. *The Clearing-up Process.*

A fixation may be called adequate when, under the particular circumstances in which it is operative, it is sufficiently accurate and sufficiently long to condition a clear perception of the object of regard. The adequacy of a fixation is a relative matter, depending not only on the character of the fixation process but also in part on central conditions, and in part on a number of peripheral circumstances, such as the amount and quality of the illumination of the object and its background, the visual angle which the object subtends, and the nature and illumination of the pre- and post-exposure fields. A complete analysis of the relative influence of these various factors would be a most desirable piece of experimental work for which considerable material is already at hand. Our own problem is less pretentious. In all these possible complications there is a relatively simple and practical problem of how rapidly under otherwise favorable conditions of normal vision adequate fixation pauses may succeed one another. Notwithstanding the relative simplicity of the problem, and its obvious practical bearings on the normal limitation of the succession of discreet visual acts, our material information with regard to it is scanty.

In every succession of fixation pauses and eye movements each new fixation involves a relatively thoroughgoing rearrangement of the retinal stimulation. Irrespective of possible objective changes within the visual field each new fixation is little short of a retinal revolution. Yet, however complete the change, the new retinal processes develop on the ruins of its antecedents. The previous stimulation passes grudgingly and unevenly, often leaving particularly stubborn residua to contest control. Conversely, each new stimulation has a latent period of incubation, and a period of more or less incompletely inhibited development, before it reaches full maturity. The history of this development, depending on the inherent strength

of the new stimulation and the strength of the residua it must supplant, constitutes a kind of monocular *Wettstreit.* On the one hand it appears as a time-consuming interference with the progress of vision. On the other it seems to be one of the most important factors in the perception of motion, and, as I believe, one of the most important sensory conditions in the spatial organization of retinal elements.

From the standpoint of the persistent past the process has been discussed under the general head of after-images, or the after-effects of stimulation. But the duration of the after-effect depends not only on the duration and intensity of the antecedent stimulation, but also on the character of the interrupting stimulus.

From the standpoint of the new stimulus, different phases of the process have found more or less adequate discussion, such as latent time, retinal inertia, the development of retinal stimulation, and the clearing-up process. The latter name is expressive and may serve very well as an inclusive term for the whole process, which, as far as I am aware, has never received systematic investigation.

While after-image and clearing-up process seem to represent the same event from opposite ends, yet there is good reason for a differentiated discussion. A well-defined after-image may persist throughout a number of successive fixations, entirely inhibiting the clearing up of some particular part of the successive fields; or again, the after-image may show a sort of intermittent existence, passing entirely unnoticed during some fixations and dominating again over its particular part of the visual field during others. On the other hand typical clearing-up periods usually pass entirely unnoticed. They are generally ephemeral disturbances of clear vision, rarely submitting to direct observation.

Physiologically speaking, the development of the new excitation is as important as the disappearance of the old. Indeed it is only by reference to a more or less inhibited new physiological stimulus that we can speak of an after-image at all. Psychologically the clearing up of the new impression is more significant since the new impression is naturally the

object of interest. Finally in the last analysis the clearing-up interval must be something more than the mere obverse of a disappearing after-image, since after-images themselves often show analogous periods of incubation and development, emerging from a hazy confusion into full brilliancy and inhibiting the development of antagonistic impressions.

As far as I am aware the clearing-up interval in normal vision was never formally recognized until theoretical considerations led to its discovery. But while it is seldom a conspicuous accompaniment of the succession of visual stimuli, it is nevertheless a real one and after it has once been discovered it may be observed with every change in the object of regard. It must, however, first be analyzed out of the complex visual process, much as an overtone, which one is trying to hear, is heard best after it has once been analyzed out of the simultaneous acoustic process to which it belongs.

A fairly good class-room demonstration of the clearing-up process may be made by beginning with an exaggerated case. If the subject will look back and forth between a well-lighted window and a wall covered with figured paper, but in moderate shadow, at each new fixation of the wall paper the true values of the different parts of the pattern will be seen to gradually emerge from a confused haze in which under some circumstances recognizable residua of the after-image of the window may be noted. The gradual emergence of the darker field is an exaggerated clearing-up process. Some practice with gradually decreasing differences of illumination of the two fields will enable the student to observe similar if relatively less conspicuous phenomena at every new fixation within any normally complex field of regard.

The fact that the clearing-up process is ordinarily unnoticed serves as a further illustration of the general principle that regularly recurring disturbances of the perceptual processes are habitually disregarded. Another difficulty in the unassisted recognition of the c.-u. p. lies in the fact that it would scarcely come to consciousness at all except in distinction either from a period of confusion or from a clearer view of the same object of regard. A consciousness of confusion however would

hardly be expected from the momentary balance of the opposing stimuli unless attention was especially directed to the phenomenon, or the period was considerably prolonged, both of which conditions are experimentally included in our demonstration. Similarly a lesser degree of clearness would ordinarily pass unnoticed as the attention is regularly claimed by the objective fact rather than by its subjective variations.

Perceived or unperceived the process involves a measurable time interval which is of theoretical as well as practical interest, since, other things being equal, it is the measure of the rapidity with which adequate stimuli may succeed one another.

§ 2. *Minimum Duration of the Clearing-up Process.*

The question of the duration of the clearing-up process is particularly refractory to *exact* experimental solution, since in the nature of things it is indefinite, and variable. There are no sharp boundaries at which the old stimulus may be said to have begun to fade, or the new to have become clear. In a more or less arbitrary way the psychophysical clearing up process may be said to begin with the beginning of the new stimulus. But in view of the purely physiological inertia of the retina this is only relatively exact; while if an inadequate fixation interval occurs between the end of the passing stimulus and the beginning of the new, such for instance as may be said to occur during rapid eye movement, then the clearing up interval, in one of its factors at least, is prolonged backward to the end of the last adequate stimulation. Furthermore, the only sense in which we could speak of an absolutely cleared up impression is that in which we would mean that the immediate psychophysical effect of a stimulation had reached its maximum. But this might occur without a real clearing up of the impression at all, as under the influence of a dominant after-image. An impression is *practically* cleared up on the other hand whenever it is differentiated from its predecessor.

The oldest and one of the least satisfactory tests for the minimal physiological clearing up time is the fusion-flicker of revolving parti-colored disks. This is traditionally regarded as a measure of the duraton of maximum positive after-images.

But in neither case is it adequate. First, because the continuous unconscious and unregulated eye movements make a farce of exact measurements of the angle velocity of the sectors.[1] But principally because the test neither gives time for a free development of the after-image independent of the beginning of new stimulation, nor is there a real clearing up of the several sectors. The colors one sees in threshold flicker are quite different from the real colors of the disk. It has always been a paradox of the traditional view of flicker that, whereas the after-effect of a stimulus increases directly with the illumination, an increase of the illumination of a revolving parti-colored disk increases the flicker. What is actually measured in flicker is the time in which the clearing-up process and the after-effect just fail to balance; and the paradox is a demonstration of the fact that after-image and clearing-up process follow different quantitative laws.

The work of Exner and Baxt on the time necessary for a visual perception is more closely relevant to our problem. Exner's experiments determined the duration of the physical stimulus sufficient to produce a just perceptible clearing up of different objects on a dark pre- and post-exposure field. He found that this depended on four factors; the illumination, the size of the object, the undisturbed duration of the after-image, and the position on the retina. With respect to the first variable, the illumination, it appears that, granted sufficient illumination, there is no lower limit to the exposure interval. Exner reduced his exposures on occasion to 1 σ. Cattell operated with .25 σ as did Erdmann and Dodge. While the electric spark used by Aubert and others is doubtless a minute fraction of these incomprehensibly minute intervals.

Unfortunately none of these results are directly transferable to the natural course of vision, since, except in comparatively rare experiences, such as the lightning flashes during a thunder storm, normal fixation pauses are never provided with dark pre- and post-exposure fields. For similar reasons the thoroughgoing experimental work of Duerr[2] is not directly

[1] Helmholtz, *Physiologische Optik,* 2 ed., p. 480, p. 488, fol. Aubert, *Physiologie der Netghaut,* p. 96.

[2] *Phil. Studien,* Vol. 18, p. 215, fol.

relevant to our present enquiry. Nevertheless the tachistoscope offers the best conditions for the solution of our problem, provided we realize its limitations. While the problem itself throws new light on tachistoscopic procedures.

§ 3. *Tachistoscopic Exposure.*

The most satisfactory exposure time in tachistoscopic experiment is doubtless not a constant but an experimental variable, whose value in any given experiment must be determined by experimental tact and expediency. The effect of its variation, on the other hand, is not a matter of tact or expediency. It is a definite psychophysical problem, capable of scientific analysis. In view of the extensive use of the tachistoscope, particularly in the study of the psychology of reading, where a study of fixation finds its most immediate practical application, and in view of the marked differences in the usage and findings of various experimenters, as well as in view of the use we must make of the tachistoscope in the studies of our more immediate problem, an analysis of the general conditions of tachistoscopic exposure needs no apology. If I may state my conclusions at the beginning, I find that the tendency to reduce the physical exposure time to a minimum wherever the aim is to present an adequate exposure of the simplest kind is a methodological mistake, based on a psychophysical misconception. It introduces unusual conditions altogether foreign to the natural fixation pause and leads, or may well lead, to a distorted analysis of the processes of apprehension; making the conclusions, in so far as they are referred to normal perception, not merely valueless but false. Obviously my thesis does not refer to those tachistoscopic experiments which aim to measure retinal inertia or the legibility of letters by the determination of the threshold exposure. In such experiments minimal exposures are the essential part of the experiment and are obviously justified. My contention is merely this, that visual perception from a threshold exposure may be and indeed must be quite a different matter from normal visual perception, and that the results of the former should be applied to the latter only where there is clear justification of the analogy.

Doubtless the main motive for working with a minimum physical exposure time is to reduce the stimulus and the consequent psychophysical process to the simplest possible terms. The experimenter seeks to isolate a single apperceptive event—to eliminate if possible all changes in the position of the visual point of regard, and in the direction of attention. Even if complete simplification of these central and peripheral elements is impossible the experimenter expects to simplify them as far as possible, to retard the processes involved, and to render them more susceptible to introspective observation. Both the means and the end are legitimate enough in themselves, our only question concerns their relationship. The immediate critical question is: does a threshold exposure simplify the consequent psychophysical process or render it more apparent to introspection. In the first place it should be noted that a minimum exposure is not necessary to eliminate reactive eye movements. As we have already seen, an average reactive eye movement to a change in the visual field is well over 150 σ. In view of the fact, however, that the eye is never absolutely at rest even during the sharpest fixation, there still appears to be some peripheral motive for a minimum exposure, to reduce to a minimum the effect of the minute fixation movements. On the other hand, the normal fixation movements are relatively slow and of relatively small amplitude. Especially is this true, if we eliminate the necessity for a new adjustment of convergence, by providing an adequate pre-exposure fixation mark. Under these circumstances an exposure interval of .1″ guarantees what is physiologically a single visual act.

Much more significant however for the reduction of the exposure time below .1″ is the psychological motive. There are no satisfactory experimental guides for the rapidity of changes of attention. There is no reliable measurement of the minimum duration of an elementary apperceptive process. The natural tendency therefore is to reduce the exposure time to the lowest practicable limit, with the express or implied expectation that the psychological processes are thereby reduced to the lowest terms. Such for instance is the motive

of the low exposure times in the tachistoscopic work of Zeitler[1] and of Messmer.[2]

This procedure, however, involves two fundamental fallacies. (1) It is not necessarily true that a low exposure time guarantees a minimum physiological excitation. Any given physical stimulus depends for its physiological consequences not only on the time it is allowed to act, but also on the pre- and post-exposure conditions, viz., on its clearing up. (2) It is never true that the complexity of the psychological consequences varies directly with the intensity of the physiological excitation.

It is noteworthy that the apparent duration of the exposure by the electric spark does not differ materially from the apparent duration of an exposure of .1″ under proper conditions of illumination. Similarly, with adequate changes either in the absolute illumination, or in the relation between the illumination of the pre- and post-exposure fields, an exposure of .1″ may have the same apparent duration as an exposure of .001″. Changes in the exposure time are commonly apprehended as changes in the amount of illumination; and conversely, changes in illumination are often apprehended as changes in the duration of the exposure.

It is further noteworthy that the threshold duration of exposure is a variable, dependent on the same conditions that determine the apparent duration of exposure, viz., on the absolute illumination of the exposed object and on the relative illumination of the pre- and post-exposure fields. An exposure time of 30 σ may be and is as truly a threshold exposure if the illumination of the pre- and post-exposure fields equals that of the exposure field, as is an exposure of 1 σ when the pre- and post-exposure fields are black. Moreover, there are excellent grounds for believing that the threshold exposure in the former case may represent a total physiological excitation in no wise longer than that in the latter case. Even if a threshold exposure time is desirable, then, I contend that an exposure time of 1 σ can never be used without artificially prolonging the

[1] J. Zeitler, 'Tachistoscopische Versuche über das Lesen,' *Phil Studien,* XVI.
[2] O. Messmer, 'Zur Psychologie des Lesens,' *Archiv. f. d. Gesammt. Psy.,* II.

positive after-image by the use of a dark post-exposure field. To insist that one must have an exposure interval of 1 σ in order to get a minimum physiological effect, without taking pains to obliterate the physiological after-effect, is to say the least inconsiderate of a considerable body of our best traditions.

My second contention is that anything approximating a threshold exposure, instead of simplifying the consequent psychological process, really complicates it and renders it more uncertain. This is indicated, first of all, by the most obvious results of reaction experiments. The minimum exposure of a word increases the speech reaction time indefinitely to infinity. We may well use the results of reaction experiments with caution, but, if they mean anything at all, a delayed reaction of two seconds under otherwise similar circumstances must involve a more complex central process than one from 300–400 σ. Unless we surrender all commonsense to the scientific abstraction of a differentiated apperceptive and assimilative process, it is absurd to maintain that increased reaction time represents simplified mental processes. My contention is supported further by the familiar facts of normal perception. It is not the clear and distinct visual impressions of bright sunlight that suffer the most 'assimilative' distortion and falsification. It is those impressions which just differentiate themselves from their background, whether on account of defective illumination, deficient size, or minimal exposure that lend themselves best to the vagaries of subjective interpretation. The illusions of the dusk are difficult to understand at noon. Analogous errors attend the use of minimal exposure intervals in reading. Words are more freely misread; letters are substituted, inserted, or omitted altogether with characteristic frequency. Or on the other hand for more careful observers, whose attention is directed primarily to the letter content of the exposure, minimal exposure emphasizes that part of the field that falls on the macula. But the real question is not whether we have to do with more or less complicated mental processes in these characteristic falsifications and selections. The question is whether an introspective analysis of perception under these unusual conditions, however valuable it may otherwise be, will

also constitute an analysis of normal perception. Obviously enough some new disturbing elements have been introduced into the process, which give rise to the increased uncertainty of the perceptual process; and the tachistoscopic experimenter must be on his guard not to pick out exactly these new disturbing factors and discover in them the otherwise hidden elements of normal perception. This, however, is exactly what Zeitler[1] does in his successive apprehension hypothesis. The subsequent detailed review of the memory image of an inadequately cleared-up word is without question a successive process. It is, however, produced by the experimental conditions not disclosed by them. The old scientific battle-cry of 'empfundene Empfindungen' may well be paralled by a new one of experimentally manufactured disclosures. Messmer[2] repudiates the successive apprehension hypothesis of Zeitler and proposes a substitute successive apprehension hypothesis, dependent on the difference of legibility of the letters. Dominant, or in general large letters are held to come to consciousness first.

Even more obviously than Zeitler's, Messmer's appears to be an experimentally manufactured disclosure. It is familiar to every experimenter with tachistoscopic exposures, since the work of Exner, that the threshold exposure varies not only with the illumination and the length of the after-image, but also with the size of the exposed objects. The threshold exposure tends to isolate the larger letters or to exaggerate them —exactly as Messmer and his keen observers find to be the fact. But the exaggeration is a product of his experiments, not their discovery. The apparent selective emphasis of the larger letters in a group is a beautiful experiment in threshold exposures.

In support of Messmer's contention it may be urged that the temporal advantage of the large letters discovered by the tachistoscope is a real one, even in the longer exposures of normal reading. Namely, in imagination at least, each full reading pause of from 100–500 σ must have had at its beginning a moment when the threshold stimulus with all its selective emphasis was approximated. This is Messmer's own

[1] 'Tachistoscopische Versuche über das Lesen,' *op. cit.*
[2] 'Zur Psychologie des Lesens,' *op. cit.*

contention; but it rests on a false hypothesis of the development of visual excitation. Threshold stimuli are much less rapid in their physiological development than stronger stimuli, and it is inevitable that before the excitation produced by the first moment of stimulation could have fully developed it must be overtaken by the excitation produced by the subsequent adequate stimulation. My real contention after all is against the attempt to transfer the effects of minimal exposure to the normal processes of apprehension. Conversely, I believe that the only exposure, whose results will apply directly to normal processes, is that which, under the given experimental conditions of illumination, will permit a full and uniformly cleared-up visual impression. For this exposure I believe we still have a right to the superlative implied in the word 'tachistoscopic,' since we refer not to the most rapid excitation but to the most rapid *vision* which is really cleared up and adequate. To discover what duration of exposure will guarantee such vision is a part of our study of the clearing-up process.

§ 4. *Influence of the Pre- and Post-exposure Fields.*

Two general demonstrations of the influence of the pre- and post-exposure fields on the clearing-up process may serve as an introduction to the more detailed investigation.

Experiment 1.—If an even white field is exposed on a pre- and post-exposure field half white and half black the minimum duration of the exposure that conditions the perception of an even white field will be the physical condition of a maximum clearing up of the white field, which for obvious reasons is not to be confused with the maximum excitation. With whites of moderate intensity the threshold of exposure will lie below 1 σ. With a threshold exposure, however, the exposed white field is not seen as white at all. It barely distinguishes itself from the black pre- and post-exposure field, as a gray. The minimum exposure is incapable of giving a cleared up impression of the white object. Indeed as compared with the neighboring white which remains constant during the entire experiment, a maximum clearing up is only accomplished by an exposure of from .5″ to several seconds, according to the duration of the pre-exposure fixation.

EXPERIMENT 1.

Pre-exposure	Exposure	Post-exposure
white black,	white,	white black.

Experiment 2.—If the whole pre-exposure field is black, while the exposure is uniform gray and the post-exposure field is white an exposure of 50 σ may utterly fail to reveal any trace of the gray. That is, the gray exposure, a duration of 50 σ, is lost in the clearing up of the white post-exposure field.

EXPERIMENT 2.

Pre-exposure	Exposure	Post-exposure
black	gray	white.

Frappant and suggestive variations of this second experiment may be made with colored exposures on colored pre- and post-exposure fields. These variations seem to offer striking demonstrations of the degree of relationship between colors. Accurate results, particularly with respect to introspection, require some form of exposure apparatus giving simultaneous exposures of the entire exposure field, with full control of duration and illumination. The only apparatus guaranteeing satisfactory conditions, in which there is full control of the illumination of the pre- and post-exposure fields, and the possibility of wide variations in the length of exposure without changing any other of the conditions of exposure, is the transparent mirror apparatus.[1] Plate II. shows the tachistoscope in position. The ordinary fall exposure apparatus is particularly objectionable for use in these experiments because of the width of their exposure field, and the obvious passing of the shutter edges in long exposures.

The second experiment seems to me of especial interest, in view of a recent discussion of the illusion of clear vision during eye movement. The demonstration that a fusion of diverse stimuli may be exposed 50 σ without clearing up, when the pre- and post-exposure fields consist respectively of the colors which are combined in the fusion exposure, is directly analogous to the ordinary lack of perception of the fusion of the field of view during short eye movements.

[1] PSYCHOLOGICAL BULLETIN, Vol. IV., pp. 10–13.

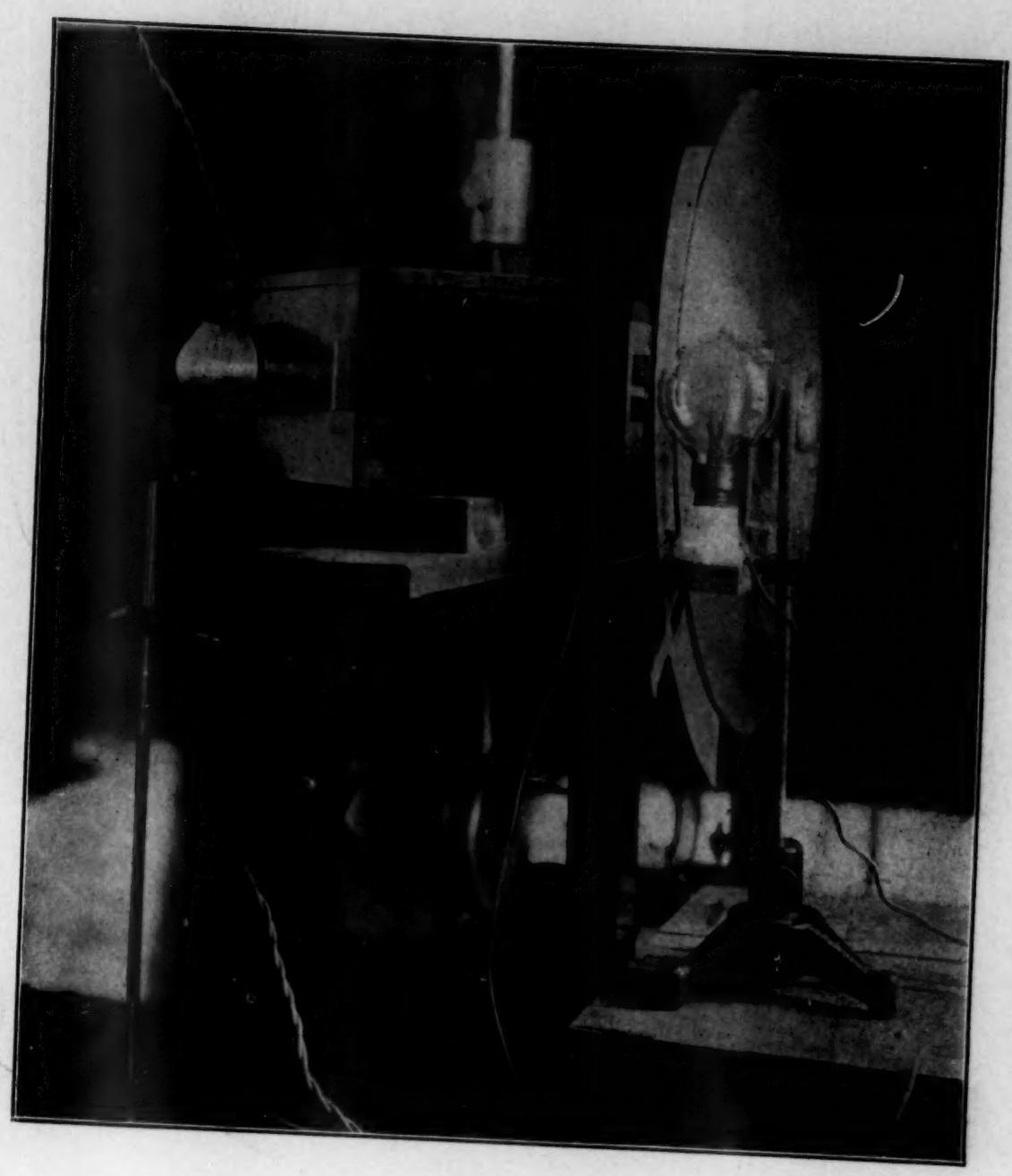

PLATE II.

THE TRANSPARENT MIRROR TACHISTOSCOPE AND PENDULUM SHUTTER.

While these experiments clearly if roughly indicate the influence of the pre- and post-exposure fields on the clearing up of an exposure, closer experimental determination of this influence is limited only by the theoretical and practical interests involved. Partly from obvious ulterior reasons of special interest, and partly because I believe that the psychology of reading offers exceptionably favorable experimental material for the study of the general processes of visual perception, the following, more detailed inquiry is largely based on tachistoscopic reading.

For reasons already given the apparatus was my binocular transparent mirror tachistoscope. The shutter was a heavy second pendulum with concentric disks attached to its axis. Exposure under these circumstances was noiseless, and might occur isolated or in groups of any prearranged number at intervals of one second.

Starting with an equal illumination of the pre- and post-exposure fields, words of the style of type used in the PSYCHOLOGICAL BULLETIN, exposed for a total legible interval of 20 σ (10 σ of which was maximum illumination) were seen as illegible faint gray masses. Succeeding exposures at intervals of 1″, occurring as the pendulum was allowed to swing back and forth, made an examination of the various parts of the gray word masses possible but they utterly failed to clear up as wholes. Even when a word was suggested, that seemed to correspond to the general appearance of the gray mass, there was no corresponding general clearing up. In successive exposures, however, two or three letters seemed to flash out somewhat more distinctly from the gray mass. These selected letters were not always together but they seemed to be at or near the point of regard. While they were not always the so-called dominant letters, the latter were conspicuously apt to appear. The phenomenon is in direct agreement with Messmer's keen observations. In the present instance, however, it was clear that the cause was a differential threshold for the larger objects lying near the point of regard.

Three variations of the second experiment gave sufficiently cleared-up impressions to make the words legible. The words

became legible if they were increased in size, though the differential emphasis on the larger letters still remained. Words of the same sized letters were legible if the exposure was increased to 30 σ; or again, without changing the exposure time, if the relative illumination of the exposure field was slightly increased.

These results remained identical for numerous series of control experiments made at various times of the day and night, and extending over a period of a year and a half. Unfortunately no one of sufficient training was available for a complete series of control experiments. But a number of partially completed series made it clear that the initial subthreshold exposure time of 20 σ was of individual value. All the inexperienced subjects demanded a longer exposure. Otherwise the above findings seem to have general validity.

The differential emphasis on the larger letters has already been considered. The differential illumination of the exposure and the pre- and post-exposure fields demanded further investigation. It was impracticable with my present apparatus, without changing the form of the shutter, to so far reduce the exposure time as to reach the threshold exposure with a dark gray pre- and post-exposure field. That has already been accomplished by Exner, Cattell, and others. On the other hand, the relative inhibition of dark and light pre- and post-exposure fields to the clearing-up process was directly in line with our main inquiry.

Experiment 3.—Following up this special problem I divided the pre- and post-exposure fields into a dark and light area, as in experiment 1, by covering half of the field with a piece of ordinary commercial black bristol board. For the exposure object I took the phrase 'week after week.' The line of division between the light and the dark part of the pre-fixation field passed through the 't' in after. Starting with an exposure time of 20 σ, I increased it by gradual stages to 500 σ carefuly noting the variation in the appearance of the two halves of the exposure object. At 20 σ only that part of the phrase that fell on the black pre-exposure field was sufficiently cleared to be legible. The word appearing on the

white pre-exposure field was only a faint gray mass, as in experiment 2. As the exposure time gradually increased the word on the black field became clearer and sharper, while the previously illegible word on the white field gradually cleared and became darker. At no time was there a sharp transition between the appearance of the words in successive exposures. Not until the exposure time reached 120 σ was it possible to speak of a full clearing up of the word on the white field. With that exposure time, the letters on the white field, and their background on the black field, seemed to clear up naturally. The only word whose legibility was at all disturbed at that exposure time was the word 'after,' half of which it will be remembered fell on either field. In spite of the fact that both sides seemed to clear up there was still a marked difference between them. There was a marked difference in the apparent time of appearance and disappearance, and there was persistent difference between the blackness of the letters of the two words and the whiteness of their respective grounds. These differences persisted, though to a less marked degree, as the exposure was gradually lengthened to 500 σ; beyond which the experiment was not carried. The results of experiment 3 show that an *absolute* clearing up of an exposed word does not occur even with an exposure of 500 σ. The length of exposure giving an adequate clearing up varies with the illumination of the pre- and post-exposure fields. The greatest inhibition arises from a combination of light and dark elements in the pre- and post-exposure field of a single word.

Experiment 4.—The question whether the pre- or the post-exposure field exercised the greater inhibition on the development of the clearing-up process is capable of experimental investigation.

Again using the same arrangement of exposure apparatus and pendulum shutter, a set of experiments was made under varying exposure time with the following arrangement. During one half of each swing of the pendulum the pre- or post-exposure field was completely darkened. This gave the following arrangement at each complete oscillation of the pendulum:

EXPERIMENT 4.

Pre-exposure	Exposure	Post-exposure
out—black	word	white
back—white	word	black

That is to say, each double swing of the pendulum gave two full exposures of the word, preceded by black and followed by white on the out swing, and preceded by white and followed by black on the return. If the exposure time was reduced to 30 σ, which was the minimum exposure giving a clearing up to the point of legibility, a curious phenomenon was observed. Each swing of the pendulum in the direction black, ex., white gave a legible exposure. Each swing in the opposite direction, white, ex., black gave an illegible exposure. This was directly contrary to expectation, but the fact was unequivocal. Observations with increased exposure time corroborated the previous results. The exposure in the direction black, ex., white was regularly clearer, as the pendulum swung back and forth, than the exposure in the direction white, ex., black. The results were independent of the position of the point of regard, and no voluntary control of the attention could change them. On the other hand, when the exposure field was made brighter than the white pre- or post-exposure field, the reverse direction became more favorable. This gave the clue to a plausible explanation of the phenomenon.

Assuming in the first instance that the black of the letters equals the black of the pre-exposure field, and that the white of the letter background equals the white of the post-exposure field, then the progress of the exposure, black, ex., white, conditions a clearing-up process as follows: The letters as they appear are a direct continuation of the black of the pre-exposure field, and they stand out in relief as the pre-exposure black passes through gray into the cleared-up white of the post-exposure field. Through the entire clearing-up process the letters stand out darker than their surroundings, on account of the initial delay in the clearing up of the white, which they condition. In the reverse order from white to black, since the entire black stimulus is apparently shorter than the white, the duration of the slight differential advantage in the clearing up of the black, corresponding to the letters, will be shorter;

and there will consequently be an exposure time when this virtual shortening of the effective stimulus will reduce it below the threshold. When, on the other hand, the illumination is such that the exposure is brighter than the white or the post-exposure field, the letters also become lighter than the black of the pre-exposure field. They will consequently tend to perpetuate themselves into the black post-exposure field as lighter elements, while they become less distinguishable from the simple clearing-up gray, in passing from black to white.

The experiment indicates how complex the conditions of the clearing-up process really are. In general, however, one can say that the relatively slight differences between the two arrangements of the black and white fields indicate that, the other conditions being equal, the influence of the pre- and post-exposure fields on the clearing-up process are approximately equal, even when their mode of action is different.

In a measure, all these experiments, with relatively simple pre- and post-exposure fields, present unnatural or unusual conditions rarely or never found in the succession of fixations in normal vision. In most instances in normal vision the visual field antecedent to, as well as that consequent to, any given fixation will be of approximately the same complication as that of the selected fixation. Now we already have some evidence that the complication of the pre- and post-exposure fields delays or inhibits the clearing up of the exposure. Direct experiment discloses an irregular variation of the clearing-up process consequent to a regular increase in the complication of the pre- and post-exposure fields.

Experiment 5.—A single horizontal line, so placed in the pre- and post-exposure field that a word is exposed directly where the line was, will render uncertain and indistinct the otherwise adequate exposure of 30 σ. Several additional horizontal lines, however, so oriented as to occupy the entire area to be covered by the exposed word fail to increase the disturbance. Indeed on a background of five horizontal lines covering the entire area of the word, the latter appeared not only plainer than with the pre- and post-exposure complication of a single line, but it was clearer than without any complica-

tion at all. A group of vertical lines, on the other hand, spaced about the same as the letters, almost completely obliterated the word. Only occasionally, and then after a succession of exposures, was there a suggestion of word form or individual letters. Not until the exposure time was increased to 50 σ did the clearing-up process approximate that which was conditioned by an exposure of 30 σ on a plain white pre-exposure field. The paradoxical character of these results is increased by the close observation of a word clearing up from a complicated pre-exposure field, during a long exposure. It was very clear that a dark line on the pre-exposure field really facilitated the clearing up of parts of the letter that crossed it. A still more serious interference with the clearing up of a word was found to be the complication of the pre- and post-exposure fields by an irregular tangle of lines. But again it was found that too great complication entirely reversed this result. The probable explanation of the decrease in the effect of too highly complicated fields is that the extreme complication acts like an even gray. The most pronounced interference with the clearing-up process of an exposed word that I was able to produce, without decreasing the illumination of the exposure, was by a pre- and post-exposure complication of exactly the same general character as the exposure, *i. e.*, by a word.

Experiment 6.—The following table shows the effect of pre- and post-exposure complication by a mirror image of the word 'explanation.' Each word was exposed in two ways, lettered, respectively, *A* and *B*. *A* is the result of the first single exposure. *B* is the result of a series of several exposures, succeeding each other at intervals of 1″. *T* gives the total time of legibility, which is regularly 10 σ longer than the time of maximum illumination.

$$T = 48\,\sigma.$$

1, Substance,	*A*, No clue to what the word might be.
	B, The word substance came to mind without conscious preliminaries, as a sort of inspiration. It was incapable of verification in successive exposures.
2, Paragraphs,	*A*, Nothing.
	B, The word paragraphs came in the same way as in No. 1. It was incapable of verification in subsequent exposures.
3, Verity,	*A*, Nothing.
	B, The word reality came as above. Subsequent exposures verified only the *ty*.

4, The articles, *A*, Impression of two words.
B, No inspiration.
5, Accuracy, *A*, Nothing.
B, 'Possibly emotion.'
6, In, *A*, Nothing.
B, 'In, doubtful,' not verifiable.
7, Extended, *A*, Nothing.
B, Nothing.
8, Suggestion *A*, Nothing.
B, 'Suspicion' not verifiable.

$T = 70\,\sigma$.

Same complication of pre- and post-exposure fields.

1, Fundamental, *A*, 'Fundamental' tentative
B, Successive exposures do not give cleared up impression.
2, Discussion, *A*, Nothing.
B, 'Discussion' verifiable on successive exposures.
3, Degree, *A*, 'Degree very plain' (the word was half a letter too low).
4, Thought *A*, 'Thoughts.'
B, Failed to clear up.
5, Judicious, *A*, Jealous.
B, Judicious.

As the time was reduced below $70\,\sigma$ the inadequacy of the exposure became more and more obvious. But even $70\,\sigma$ was obviously too short for an adequate clearing up. An increase of the exposure time under otherwise identical conditions gave the following results:

$T = 170\,\sigma$. Unnecessarily long, fully cleared up.
$T = 125\,\sigma$. Fully cleared up appearance. No ambiquity. Every word read correctly and without hesitation.
$T = 80\ \sigma$. Scarcely better than $70\,\sigma$.
$T = 100\,\sigma$. No mistakes but the words do not fully clear up. The exposure seems uncomfortably short.

Notable differences resulted from changing the size of the letters of the exposed word. With the same complication that made a 12 pt. word illegible when $T = 70\,\sigma$, a 24 pt. word was entirely legible and fairly cleared up, and even a 16 pt. word was legible.

§ 5. *Normal Interference of Successive Fixations.*

Obviously none of these experiments quite corresponds with normal vision. The complication of two normal fixation fields

is seldom identical; and secondly, every natural fixation pause is preceded by a moment of inadequate neutral stimulation of from 15 σ–100 σ duration (the duration of the eye movements) during which the after effect of the pre-fixation field is somewhat abated; while the same period at the end of the normal exposure must elapse before the operation of the post-exposure field. Obviously some control was necessary before the results of the previous experiments could be transferred to normal vision. To that end I constructed an escapement exposure apparatus, in which each new exposure was produced by the rapid movement of the words into place behind a narrow slit. This movement of the field of view is the best available experimental substitute for the moments of inadequate retinal stimulation due to eye movements. The escapement exposure apparatus makes a fairly satisfactory arrangement for the exposure of any small objects.

In the form used by me, the apparatus consists of a light wooden framework, carrying a screen towards the observer, which covers the mechanism and the word-disk with the exception of the one word which is being exposed. Behind the screen is a light metal sector which carries the word-disk. Equally spaced teeth on the periphery of this sector engage an escapement so that whenever the free end of the escapement is depressed one and only one tooth may slip past. As the words on the word-disk have the same angular distance as the teeth on the periphery of the sector, each depression of the escapement brings a new word into place before the aperture in the screen. The time of exposure is determined by the rapidity with which the escapement is depressed. The whole apparatus was bent up from sheet tin, and works admirably. The springs actuating the various parts were empirically adjusted until there was no apparent vibration of the word, as it came into place; and until there was the least possible suggestion of motion as each new object was sprung into position. The escapement was actuated by adjustable teeth, attached to the disk on our second-pendulum. Since these teeth could be of any number and any desirable distance apart, provided the entire series did not occupy more than 750 σ, the number and

duration of the exposures was capable of considerable experimental modification. A tuning fork record near the periphery of the disk permitted rapid experimental variation of the exposure. The results with the escapement apparatus showed that the results of experiment 6 are applicable to normal conditions with comparatively slight correction. An exposure interval of 50 σ gave four correct readings out of ten. In one of the four the sequence was inadvertently known. In all of them the number of possible words was known. In all four the same curious phenomenon was observed that was described in experiment 6. The word came to consciousness as a sort of inspiration, apparently without any real justification in the observed facts. This same experience was confirmed by a number of visiting psychologists and is rather suggestive, not only on account of its relation to experiments to be described later in connection with peripheral vision, but also because of its relation to the current discussion of the subconscious. There are numerous familiar mental phenomena that suggest a similar explanation of origin in partially cleared-up sensory impressions, but there are comparatively few that so lend themselves to direct experimental analysis.

An exposure time of 60 σ permitted correct reading of all the words provided only one was exposed at a time. On the other hand an exposure interval of 100 σ was necessary to get a fairly cleared-up impression, and this was the approximate threshold for a succession of 3–4 exposures.

Notwithstanding the general agreement with the previous experiments, the results indicate that the more natural conditions of exposure involving a neutral inter-exposure interval, and different pre- and post-exposure fields have a real if a relatively small advantage over the conditions obtaining in the transparent mirror tachistoscope. They add to our previous discussion the new fact that in a succession of equally timed exposures the threshold of adequate exposure is materially raised; and conversely, that the relative completeness of the clearing-up process becomes more and more imperative. With equally illuminated and equally complicated fields, as, for example in reading, 80 σ to 100 σ is the minimum adequate exposure.

Two further control experiments completely justify this conclusion. It is possible to make a voluntary sweep of the eye from a given fixation point out and back, so rapidly as to preclude all clear and distinct vision during the eye movement. The operation may require some practice. Similarly it is possible to make a succession of eye movements in a given direction so rapidly as to prevent clear vision during the movement. In cases where the field just clears up in the former case there is regularly an illusion of motion.

It was not difficult to obtain a photograph of the eye movements under these conditions. While a detailed examination of the record would be of little value since none of the fixations are even relatively good, the record shows that real fixation pauses may be too short to give a cleared-up impression. Further data in the matter more immediately capable of interpretation is found in the records of the most rapid possible reading (Fig. 2), which are discussed later more in detail. Huey and Dearborn both find reading pauses well below the recorded reaction time of the eyes. This is clearly confirmed by my records. The fact suggests a considerable complication of the visual processes which constitutes the central problem of our third chapter. Our immediate interest in the facts abstracts from the complex apperceptive process and fixates the immediate visual pre-conditions. Obviously it is really impossible to separate the two factors. No reading process can exclude the reproductive processes which are the universal condition of developed perception. Indeed their presence is implied in every judgment of an adequate fixation. But while the experimental elimination of this factor is impracticable, its temporal effect on the reading pauses may at least be reduced to a minimum by complete familiarity with the text and by the effort to read with the utmost rapidity. Even under these circumstances the shortest adequate fixation pauses in reading are between 70 σ and 100 σ. See Fig. 2. And these are not infrequently accompanied by a feeling of dimness. With the shortest fixation pauses there is conspicuous lack of a full clearing up. While accurate records are probably not available to most of my readers the ability to move the eyes faster

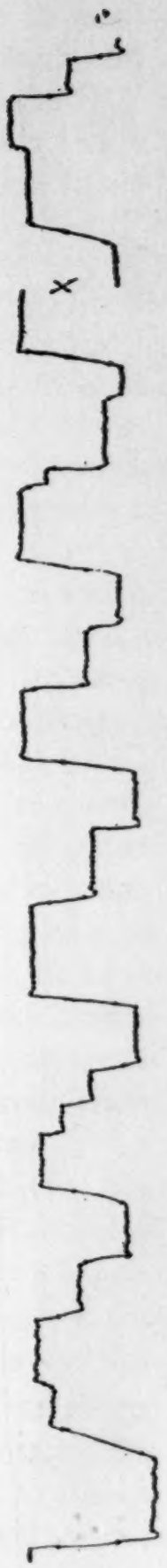

FIG. 2.

RECORD OF THE EYE MOVEMENTS AND FIXATIONS IN READING.

FIG. 2 is a drawing one and one half actual size, from a record of the eye movements during the most rapid possible reading of several lines of familiar text. The record should be read from below up. Vertical lines correspond to fixation pauses. Oblique lines correspond to eye movements. The drawing was made by projection and represents fairly accurately the appearance of the record except that the dots appearing in the oblique lines should be much fainter and much elongated.

The waviness in the early fixations is instrumental and resulted from a jarring of the apparatus.

The absence of head line makes it impossible to demonstrate accurately the meaning of some of the lesser fixation movements, but the particular series of investigations which these plates subserved was temporal and not spatial. Each dot represents a complete phase of the alternating current—and approximately 1/60″.

Fixations 1 and 2 are preliminary control fixations, respectively, at the beginning and end of the line.

The return from 2 to the beginning of the line is interrupted by a relatively short fixation too far along in the line; a corrective movement brings the extreme end of the line into view.

The first two lines have four fixation pauses each, showing the same tendency which Dearborn noted to make more fixations in the first part of a passage.

The drawing clearly shows the little hooks at the end of the long sweeps, as well as at the end of many of the shorter sweeps. Huey first called attention to these overshoots, but for some time it seemed doubtful whether they were real eye movements or were due to the momentum of the recording lever. The less delicate lines of my earlier photographic records failed to show them, but they appear to be regular characteristics of my reading records. I have counted them as a part of the fixation pause, but this procedure is open to serious question.

The shortest fixation is fixation 2, line 5. It lasted cr. 40 σ.

than the print clears up will be capable of easy proof. The form of the eye movements under which the phenomenon occurs must be taken from the available records.

Both experimentally and practically the operation of the visual clearing-up process precludes a succession of adequate visual fixations under .1″ each.

Chapter III. The Complication of the Visual Processes During Fixation.

§ 1. *Eccentric Vision in Reading.*

The records of reading from which at the close of the last chapter we drew illustration and corroboration of our generalization concerning the minimal clearing-up process, correspond as poorly with the available data concerning ocular reactions as with the results of reading reactions in general. It will be remembered that my reaction time for ocular movement in response to a peripheral visual impression is 165 σ. This is complicated in reading at least by the complex apperceptive processes which condition the apprehension of the words as such, their contribution to the sum-total meaning consciousness, and the more or less definite mental and physical reactions to that meaning. There would be obvious difficulty in crowding all this into a period of 100 σ. But the utter absurdity of the attempt to crowd it into the minimal pause of 40 σ relieves us of the obligation. We are forced to postulate a concurrent complication of the psychophysical processes of perception, extending through several fixations. The facts demand an attempt to submit the whole tangle of concurrent processes to experimental analysis. But the main question of our present discussion is the relation of the comparatively short moments of clear vision to the total perceptual process.

It is a significant fact, first noticed by Dearborn, that the initial fixation in reading a passage tends to be relatively long. My own records, as illustrated by Fig. 2, confirm his generalization. But in view of the probable complication of the visual processes, it seemed worth while to enquire more specifically just how long the initial fixation will be when a group of words suddenly appears at or near the area of clear vision, without the usual peripheral and conceptual, pre-fixational preparation for its perception. Such an experiment should furnish reliable data for determining where in the complex

elaboration of the sense material the normal fixation pause really belongs.

To that end the recording apparatus was arranged substantially as illustrated in Fig. 1, and Plate III., for the ocular reaction experiments. The falling screen shutter exposed a word or group of words simultaneously with the opening of a path for the recording beam of blue light. The mouth bit was placed in a brass tube whose far end was covered by a rubber diaphragm. The slightest vibration of the latter would break an electric circuit which would in turn release a screen, shutting off the light, and thus ending the record. These records have the same general appearance as the enlarged reaction record, Plate I., Fig. 3, except that the whole record is in some cases much longer. When two words were exposed sufficiently separated to necessitate a separate fixation act for each word, the fixations of the first word are universally somewhat shorter than the voice reaction to that word. The eye is through with the first word and has gone on to the next before the first word is spoken. The exact relations in a typical record are as follows:

From the exposure to the fixation of the second word, two inches to the right of the first	722 σ
From the exposure to the voice reaction of the first word which is four in. from the primary fixation mark	779 σ
The voice reaction to the first word after fixation	365 σ
The voice reaction to the second word after its fixation	357 σ

The first two values indicate that the two processes, the elaboration of the speech reaction and the preparation of the eye movement necessary to fixate the second word, are proceeding simultaneously. The speech reaction is only 57 σ later than the end of the fixation. In other words the first word was spoken after the second had been fixated for a considerable length of time. The records made under these circumstances are only four in number, but all agree in the points above stated. Few as they are they furnish confirmation for Dearborn's hypothesis to explain the relative length of the first fixation in a line; viz., that the eye in the first fixation takes a more general survey of the line as a whole than it does in the latter fixation.[1] But the records indicate that this general

[1] Dearborn, *Psychology of Reading*, p. 63.

survey is of much greater importance in the general reading process than most of us had ever imagined. The attempt to obtain some quantitative estimate of the importance of this pre-fixational vision led to the abandonment of the experiment in the above crude form and its continuation under more exact conditions to be described presently. Finally the records seem to indicate that the normal fixation pause in reading corresponds to the latter part of the unprepared fixation of our experiment. It seems probable that the normal reading pause represents a comparatively late moment in the total process of perception of the fixated object. If this be capable of substantiation, it must cause a considerable increase in the traditional estimation of the relative importance of peripheral vision.

The extent of this area of extra-foveal pre-perception together with the relative value of the pre-fixational data from different zones of the retina is capable of experimental determination. To this end I arranged the following modification of the above described preliminary experiments.

Instead of exposing the word directly at the fixation area, each word was placed at some predetermined distance from the fixation point. In each case this necessitated a reactive eye movement before the exposed word was brought to the area of clear vision. In each case on the other hand there was a pre-fixational vision corresponding to the normal reaction time of the eye. The relative effect of this comparatively constant pre-fixational vision, according as it involved different zones of the retina is the point of interest in the experiments and is given diagrammatically in the following figures.

These experiments indicate that the pre-fixational perception is in no wise limited to the immediate neighborhood of the macula. It may, on occasion at least, extend clear across the length of an 8vo line; and there is some evidence that it is not confined to the specific line which is ostensibly being read. The possibility of intelligent voice modulation in reading aloud from totally unfamiliar text is not wholly due to the advanced position of the eyes in vocalized reading; it also indicates the breadth of the extra-foveal pre-perception. While the well-known disturbances caused by right-sided hemianopsia indicate its necessity in all normal reading.

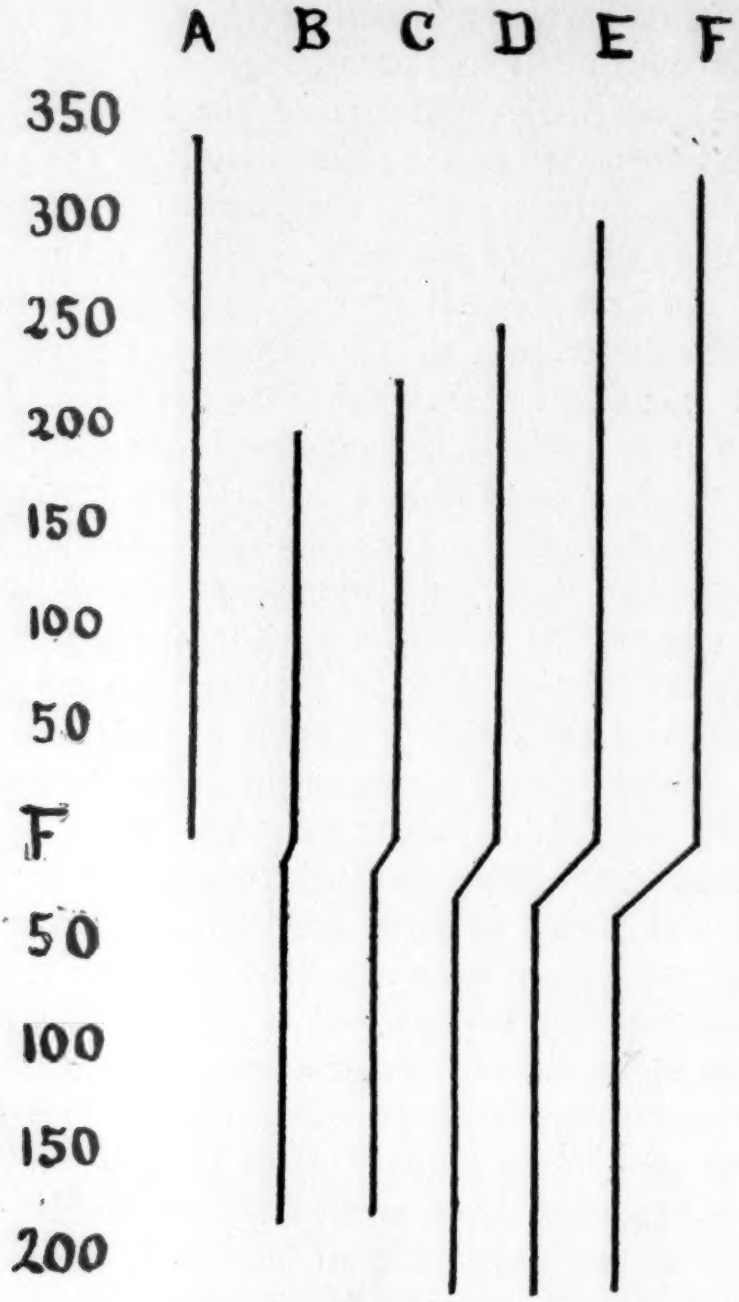

FIG. 3.

Line *A* = the average duration of a speech reaction in thousandths of a second, when a four-letter word was exposed at the fixation mark. 21 records—all included.

Line *B* shows the influence of the pre-fixational exposure in shortening the speech reaction when the word is exposed 1/2 in. to the right or left of the fixation mark. 12 records.

Line *C* shows the same effect, slightly less, when the word was 1 in. to the right or left. 6 records.

Line *D* shows the saving when the word was 2 in. right or left. 7 records.

Lines *E* and *F* show the saving when the word was respectively 3 in. and 4 in. to the right of the fixation mark. 6 and 3 records, respectively.

I venture the belief that the above demonstration of peripheral complication of the visual processes in reading is capable of the widest generalization. Nothing is isolated in the visual field merely because it is situated at the fovea. Every fixated object, even in tachistoscopic exposures is seen in a more or less complex setting. And the setting of each fixated object must be given by more or less complex extra-foveal data which are elaborated in complication with the data of foveal vision. Somewhere within the concurrent processes arise the motives for new fixations and the processes of abstraction by which alone we may speak of individual objects of regard within the visual field. What or how large the perceptual unit shall be is, sensorily considered, a matter of complete indifference. The perceptual unit is determined by variable concurrent central processes whose complex conscious correlates are expressed by the terms interest and attention.

§ 2. *Successive Elements of the Process of Perception.*

To speak of the perception of a word in ordinary reading as a successive process seems to be altogether in harmony with the facts. In general it would be difficult to conceive of a *process* in which there was no succession, and we have demonstrated experimentally that the perceptual processes in reading may begin one or more fixations before the specific word is directly fixated—if indeed it ever is fixated. But to insist that this succession in the perceptual processes necessarily involves a successive apprehension of letter units seems to me as absurd as would be the contention that we cannot perceive a house without a successive apprehension of the clapboards and shingles at least the 'dominating' ones. To insist on the other hand that such a successive apprehension of the letters is impossible would be equally absurd, provided they fell within the area of relatively clear vision. But the mental clearing up of these relatively minute perceptual units will depend on definite concurrent central complications in which these particular units are the objects of interest and attention. One may very readily demonstrate the effect of such central complications by suitable tachistoscopic experiments. But to contend

that the experimentally manufactured facts are a phenomenon of ordinary reading is as futile as to contend that the process of the perception of words would be explained if one could say that words are perceived by a successive apprehension of their parts. The real problems of perception, the determination of the units of the perceptual process, the concurrent perception of groups, the relation of the immediately fixated to the peripherally seen, and the final organization of the whole into larger units, would be merely shoved back by such an explanation from word units to letter units. The real succession in the perceptive process would remain undiscovered and unsought. The process by which new perceptual units come to take the place of old ones in normal life shows how definite is the central complication on which the unit depends. It takes long training to 'see' the minutia of technique. It takes long training to dissociate angular perceptual units and 'see' them as lines instead of as areas, but it can be done. And while the process may be associated with definite mannerisms of visual fixation, it is a peculiar fact that once the unit is established by a sufficient course of training, it may remain uneffected by chance variations in the clearness of details or in the exact position of the point of regard.

What the central conditions are which determine, in any given instance, the perceptual units is at present a matter of conjecture and interpretation rather than of experimental demonstration. Judd holds that the organization of sensory data into units as well as its organization in general is primarily a motor fact. In so far as my own process of reading is concerned I believe his contention is justified by the facts, but the immediate motor organization in my case is articulatory rather than visual. Whatever the elements are and however they are conditioned as units, our present interest centers in the process by which the new, relatively indefinite, peripheral material enters the complex perceptual process, while the fixated unit is clearing up and receiving its last sensory impulse to clear perception.

The nature of the data involved in the pre-fixational vision may be determined by direct observation. In the first place it

is not individual letters. From any given fixation point individual letters cease to be clearly seen at a distance much less than we found reason for asserting the pre-fixational beginning of perception. The only thing that is distinguishable half or two thirds across the page is a vague outline of the word, a vague word form, if, in spite of recent criticism, we may still continue to use the term without the formality of a justification. In many cases, it would be altogether impossible to recognize the word from this vague outline. In all cases its perception is doubtless delayed beyond the normal perception time for objects at the macula. The question is: What can that vague outline do towards the initiation of the subsequent clear perception? I conjecture that as a stimulus its influence is general rather than specific. I conjecture that the pre-fixational stimulation is a general stimulation of a considerable group of verbal residua. And prolonged observation of the peripheral field supports the hypothesis, since from the shadowy outlines of the peripherally seen words a succession of words may tentatively arise, more or less similar in general appearance, which we may test out by comparison with the peripherally seen word until we find one that fits. In the normal reading process there is no time for any of these suggestions to be tested out; they doubtless never pass the stage of mildly aroused residua, belonging to a general group. As in subsequent fixation the peripherally seen word comes to the area of clear vision I conjecture that the inhibitory function of clear perception becomes more prominent, shutting out of the competition all of the residua aroused by the more general peripheral stimulation except those further stimulated by the new, more definite details. Whereas there is absolutely no evidence in normal reading for a successive constructive synthesis of words out of their letters, I believe there is evidence for a more or less gradual selective discrimination in favor of the more fitting components of a more or less general stimulation. The processes of stimulation and inhibition that characterize all neural action are doubtless no more exactly simultaneously balanced in visual perception than in ocular motion.[1] And there is certainly some evidence for the as-

[1] Compare *Yale Psychological Studies*, Vol. 1, No. 2, p. 386, fol.

sumption that the progress of the development of an individual nervous excitation follows the direction of the development of nervous impulses in general, *i. e.*, from the general to the specific. Aside from the purely visual pre-fixational stimulation of the verbal residua, there are unquestionably other pre-fixational factors entering into the pre-perceptive arousal of verbal residua. Most conspicuous of these other factors is the development of the meaning consciousness. At any given point in the reading of non-technical, familiar discourse there are relatively few words that would serve as fit continuations of the matter already read. This development of the sense may be our only clue to missing words, to misprints, and to partially blurred words. It may enable us to read aloud words on the next page before it is turned, and may even on occasion lead to the illusion that the word has been printed twice, at the end of one page and the beginning of the next. Similarly familiar phrases may be pre-perceptually completed; and undoubtedly rhyme acts in a similar manner. Any of these factors, and there are doubtless others almost as important, may operate chiefly as a general stimulus or chiefly as an inhibitory selective influence on a general stimulation already operative. But whatever the origin of the stimuli, whether objective or subjective, whether visual or interpretative, it is evident that some are more general than others; while in some the inhibitory restrictive elements are more prominent. It is evident, furthermore, that the pre-fixational, peripheral visual stimuli belong to the former class, rather than to the latter.

This theory of the value of the peripheral processes serves in a measure to explain the length of the reaction time of the eye. Contrary to our psychological tradition, it is not essential to the best development of the visual percept to transfer a peripheral stimulus with the utmost celerity to the fovea. The peripheral vision itself has a real part to play in the total process and to inhibit the peripheral stimulation before it had visually cleared up would be to lose that value. Doubtless this factor interacts with the necessity for some elaboration of the motor impulse that shall bring the new object of interest to the area of clear vision. Unquestionably the peripheral

vision is sometimes satisfactory enough in itself without demanding clearer vision. Such is undoubtedly the case in reading for zones bordering on the macula.

Just as the new visual stimulus, breaking into the pre-perceptual field must last long enough for an adequate clearing up, so the pre-perceptual material breaking into the mental complex may pass gradually, by a selective inhibitory process, into an adequately cleared-up consciousness of the object itself. On the other hand there is abundant evidence that it may operate to modify the general conscious complex into which it enters without ever consciously clearing up at all. Jastrow's experimental evidence of the influence of invisible lines is to the point. Moreover, the peripheral pre-fixational factors, under ordinary circumstances, never clear up in consciousness as independent percepts. The disturbing lines in an illusion are never immediately recognized as such without some sort of experimental analysis. The peripheral setting of a visual object modifies our attitude towards it without necessarily clearing up in consciousness. Furthermore, there is some evidence that even within the object of regard, foveal or near foveal elements may operate to modify the total resulting consciousness without any proper clearing up in consciousness. An illustration of the fact occurs in the consciousness that an exposed word was incorrectly spelled, without a corresponding specific consciousness of what or where the error was. Doubtless the individual letters operate in a similar way in normal reading.

Such non-independent processes, whether peripheral or central, have been variously named subconscious, subliminal, or fringe phenomena. They are all included under the purely descriptive, generic term, uncleared processes. This includes the faint subliminal stirrings, as well as the otherwise adequate stimulation which encounters inhibitory central resistance in the form of preoccupied attention. It includes, furthermore, those stimulated memory residua that obviously modify our appreciation of present conditions without appearing in the form of memory images. Among these latter clearly belong the individual word meanings and their immediate con-

tribution to the total meaning consciousness of what we read. The concept of the uncleared processes is as old as Aristotle and recurs in ever new forms in the history of psychology. That presentations have a period of latent incubation, a period of clearing up, and a period of elaboration, receiving elements from the unevenly disappearing conscious field which it displaces, and leaving more or less persistent residua to complicate the clearing up of succeeding fields; and finally that these uncleared factors coexist in a bewildering complexity during every moment of consciousness: all this is no new doctrine. My task has not been to demonstrate the obvious, but to attempt to trace some of the simpler lines of complication in the process of visual perception and disentangle some of the simpler relationships in the process of reading. Here at least the process is capable of some analysis and we can demonstrate how the total consciousness is modified, partly by the vague peripheral vision, more sharply by the clearer vision of the macula, less conspicuously perhaps but just as really by the continuance in peripheral vision of a considerable succession of previous fixation objects. In a real sense, phrase, sentence, and paragraph, episode and plot form a ' dynamic background,' or setting, for each new word complex as it clears up. And each new word complex in passing adds its modicum to the more slowly developing consciousness of meaning which in turn becomes part of the setting for still later elements. An adequate experimental analysis of these apperceptive complications has yet to be made. Consequently the relative position of the visual fixation pause in the total complex can at present be estimated only roughly as somewhere in the middle of the process, between the pre-fixational vision and the utterance, or appreciation.

§ 3. *The Determination of a New Point of Regard.*

Among the concurrent perceptual processes, the one most intimately connected with our previous investigation is the reaction by which the new peripheral object of interest is brought to the area of clear vision. The comparative length of the ocular reaction time and the brevity of some of the fixations

forced us to admit the possibility of a pre-fixational origin of the ocular reactions in reading; that is, that the impulse to a given eye movement may have had its origin at some point in the reading process more remote than the immediately preceding fixation pause. The possibility of such a predetermination of the eye movements was already demonstrated in the case of those rapid return eye movements in which the return sweep followed the outward sweep so quickly that there was no adequate clearing-up interval, cf. page 48, and consequently no possible occasion for an adequate visual stimulus for the return movement. Furthermore, all competent observers agree that in reading the eyes follow a more or less unstable rhythm, with characteristic individual tendencies. But while the number of fixations show what may pass for an habitual rhythm there is less uniformity in the extent of the eye movements between fixations.

It is a familiar dogma of psychological optics that the point of regard coincides or tends to coincide with a point of interest, but neither in the assumption of a necessary *point* of interest, nor in the assumption that the latter even if it existed would ever exactly coincide with the point of regard does the dogma correspond with the facts. On the contrary it has been shown that if the object of regard happened to be a point it could never coincide with the point of regard except by accident, on account of the persistent physiological disturbances of fixation. We have seen further that the object of interest in normal reading is practically never a point but an area, whose dimensions are determined not merely by visual expediency, but largely also by complex central conditions. While it is true in general that the point of regard will tend to lie within the object of interest, even this generalization must be limited both temporally and spatially. First the object of interest regularly becomes such because of its relation to a previous object of interest and usually it becomes such while it is still seen only peripherally. But the wide variation in altogether satisfactory fixations makes it possible that in viewing a succession of relatively small objects of interest, the point of regard should not fall on any one of them. Cases to the point are the viewing of letters, words, and groups of words, see page 22.

In all this it appears again that the precise position of the point of regard with respect to the object of interest is a matter of relatively small importance. For example a short word, like the word *object,* is visually as clear when I fixate the end as when I fixate the beginning. It is as easily apprehended from one point as from the other. Moreover, the speech reactions show no univocal variation in favor of any one fixation. It would be strange therefore, especially in view of the physiological variables which are involved, if the point of regard should fall on the same part of the word in successive fixations, and there is abundant evidence that it does not.[1] Further evidence as to the insignificance of the exact position of the point of regard in reading is seen in the entire absence of the minute corrective movements in rapid reading that always occur in any attempt to fixate a point accurately.

The previous question what determines the precise position of the point of regard at any fixation pause must be answered negatively—no one factor determines it either psychological or physiological. Positively, on the other hand, one may say it is determined (1) by the unsatisfactory pre-fixational perception of a peripheral object of interest;[2] (2) by the central habitual predisposition; (3) by gross accidental variations due to the chance arrangement of numberless physiological factors. Of these three general conditions obviously the third merely prescribes the limits within which the first two are operative. At the same time it gives some indication of the questionable value of the painfully exact determination of the position of the point of regard. However, we must admit that, within certain considerable limits of error, the successive positions of the point of regard give some indication of the extent and character of the corresponding objects of interest in the complex perceptual process. The way in which the fixation works up into the angles of a Mueller-Lyer illusion, when the illusion is most strongly seen, seems to indicate not so much the motor organization of the visual processes as rather the obvious fact that, in spite of intent and introspective assurance, the real object whose partial pre-fixational perception occasions the eye

[1] Dearborn, *The Psychology of Reading,* chapter XI.

[2] See Dearborn, *op. cit.,* p. 91.

movement is something other than the exact point of intersection of the lines. It seems to me it would be absurd to hold that the point of regard within the angle necessarily coincided with the imperfectly seen peripheral object of interest. In this case doubtless as in reading the true object is not a point but an area. While the actual point at which the point of regard rests is quite accidental, save in its general relation to the area of interest.

Similarly in reading, while the exact position of the fixation point is usually without significance as an indication of a *point* of interest, the length and number of the eye movements must be a reasonably safe guide as to the perceptual units and the completeness with which pre-fixational data satisfy the requirements for clear vision. For instance, the way in which familiar phrases are satisfactorily apprehended from a single fixation seems to indicate that in some cases the phrase itself may become the perceptive unit.[1]

[1] Dearborn, *op. cit.*, p. 86.

Chapter IV. The Organization of Retinal Elements.

§ 1. *The Function of Extra-foveal Vision in Space Perception.*

Since no object, however minute it may be, is confined during fixation to the fovea alone, and since most objects of regard actually clear up as a result of stimulation which is largely outside the macula, while even the peripheral retinal areas regularly add their part to the complication of the central processes; the function of extra-foveal vision refuses to be regarded as merely preparatory and secondary. On the contrary in at least one typical and rather exacting visual process, *i. e.*, that involved in reading, we have seen that extra-foveal vision is an integral and invaluable factor in the total product of each fixation; without which each successive reading pause would add only a relatively insignificant detail to a relatively unorganized mass of experiences. The very length of the ocular reaction bore testimony to our general contention. Even when the object of regard happens to lie entirely within the fovea, foveal vision is never isolated and distinct from the extra foveal. Ordinary observation gives no adequate clue to the position of the point of regard. Every object of regard is seen as a part of a simultaneous field. And the field, real or imaginary, is a significant factor in the perception and interpretation of every discreet object of regard. Moreover, the field is not a mere mosaic of previous foveal stimuli; for each foveal stimulation is only a part of a whole in which a distinction between the foveal and extra-foveal never appears. Physiologically and psychologically, the only functional distinction between different zones of the retina is determined anew in each act of vision, according to the adequacy of the resulting vision.

Doubtless the most universal as well as the most conspicuous relationship between the various retinal areas is their spatial organization. With one or two notable exceptions, it has been the custom in psychological discussion to view the

spatial organization of the eccentric retina from the standpoint of the relatively more important and refined organization of the fovea. Extra-foveal vision has been consistently described and evaluated by its relation to foveal vision. Certainly in the case of a single eccentric object of interest there is obvious justification for the traditional precedure. But notwithstanding the preponderance of scientific judgment, the new facts at our disposal with relation to the fixation movements and the increased significance of peripheral vision, persistently raise the question whether the traditional view is not one-sided. Are the spatial relations of the total visual field determined by its relation to the fovea, or is the object of regard not rather determined in its spatial relations by its apparent position in the total visual field? Personally it seems to me that were our judgment not influenced by the deference due to a long line of theoretical considerations we would be inclined to adopt the less popular alternative. Certainly that would correpond more closely with the facts of experience as they are amenable to direct observation. For example, the horizon is not commonly apprehended by referring to it some minute foveal object of regard, the latter is oriented rather with respect to the horizon.

In view of the facts of immediate experience, in view of the incontestible facts of ocular movement as disclosed by Stratton, and by Judd and his assistants; and finally, in view of the lack of a definite intra-foveal fixation point; the question forces itself more and more strongly upon us whether the more general traditional view of the spatial organization of the retina does not need thoroughgoing revision. The task is a serious one and it obviously does not fall within the scope of a discussion of visual fixation. But, on the other hand, the data which force the reconsideration suggest a line of acceptable reconstruction, centering in a new theory of the development of retinal local signs.

§ 2. *Retinal Local Signs.*

A satisfactory local sign must serve two functions. It must serve to differentiate some particular retinal element from

every other, and it must serve as a bond between the differentiated element and the other interrelated parts of the retina. The motor impulse, by which a peripheral object of regard would be brought to a supposed fixation point at the center of the retina, would seem to be admirably adapted to the purpose. In the amount of the necessary innervation, and in the specific combination of the eye muscles involved, each retinal point would not only be differentiated from every other point, but through the interaction of the different muscles in different degrees for neighboring points, all the retinal elements would be united in a relatively simple closed system, without duplication, and without hiatus. The theory was originally proposed purely on theoretical considerations and it maintains its position at the present time on purely theoretical considerations. As far as I am aware there is not one particle of unambiguous experimental evidence that can be adduced to its support. The closest approximations to such evidence are the familiar facts that we do move the eyes to bring objects of interest to an area of clear vision; and that there is a certain demonstrable kinæsthetic consciousness of ocular position and of ocular movement. The grossness of the errors of the latter data are evidenced by the mass of entirely unsuspected facts respecting the eye movements, that have been brought to light by better means of direct observation, and by objective registration; all beginning, I believe, with a demonstration of the fixation pauses in reading. The relatively large eye movements during the consequent fixation of minute objects of interest was first brought to light by the refined technique of recording apparatus.

In addition to these rather dubious supports is the alleged evidence that the perception of motion in the third dimension follows laws that are most easily interpreted as a perception of convergence movements of the eyes. But when it is considered that there could be no impulse to convergence except as a response to some adequate stimulus which indicated the need of convergence; and that this antecedent stimulus would furnish the most immediate and direct evidence of movement in a third dimension; and when it is further considered that

all convergence movements permit clear vision during the movements of convergence, and that consequently they are all accompanied by corresponding retinal impressions, it becomes obvious that convergence movements of the eyes have no peculiar or unusual fitness for mediating the perception of depth. And finally, when it is considered that in simple convergence movements without either of the above factors, as when a binocular after-image, sufficiently strong to persist some time after closing the eyes, shows no apparent motion in the third dimension, notwithstanding the general tendency of the eyes to relax all convergence when at rest, and even in spite of actual voluntary convergence, the alleged evidence from the convergence movements becomes not only doubtful but improbable.

While there is no direct and unequivocal evidence capable of demonstrating the traditional theory, there is a growing body of evidence of its inadequacy. Probably the most conspicuous result of all the recent exact data with respect to the eye movements is the evidence they force upon us of the utter insufficiency of the ocular kinæsthetic data. Neither the velocity nor the extent of our eye movements is directly known to us. We have no immediate consciousness of their direction. Usually we are unaware of their existence. All this evidence comes as a shock to the self-complacence with which eye movements and eye strains have been exploited by the introspectionists. But along with the necessary changes in our traditions, which must follow the knowledge that our eye movements are not adequately represented in consciousness, may come the correlate that, so far at least as the theory of motor local signs are concerned, direct consciousness of eye movement is altogether unnecessary. It is not necessary that the local sign should be at once both a psychological and an identical physiological fact. Indeed when we stop to think of it the identity of the two series in any element of our experience would be a matter of far-reaching significance, which we would not take as calmly as we acquiesce in the local sign theory. The main functions of a local sign would be subserved by the physiological facts of ocular adjustment, quite irrespective of their direct representation in consciousness. To ask for the latter in the case

of retinal organization would be to demand more than we have to be content with in many other forms of mental organization. At least we must agree that the lack of immediate consciousness is no valid argument against the operation of the motor impulses in a differential organization of the retinal elements. But the theory loses its supposed introspective support; and the original form of its statement must be modified to eliminate the word *sensations* of movement. Adequate sensations of eye movement simply do not exist. Whatever the motor factor in the spatial organization of the retina may be it comes to consciousness not as sensations of movement but as spatial relations.

The question whether the oculo-motor impulses, as we now know them through the medium of exact registration, are adequate to account for the spatial organization of the retinal elements seems to me to represent the real crux of the problem. And there is a considerable body of cumulative evidence, without a single negative element, that they are not. The first evidence in this connection was published by Dearborn.[1] He found that the average error in the motor impulse by which the fixation of a peripheral object was brought about was approximately ten times as large as the local discrimination, when both were measured at a point on the retina 40° from the point of regard. McAllister found that there was a disproportionately greater error in the movements to fixation when the object is only 10° removed from the point of regard.

But in addition to this we must also take into consideration the changes that have taken place in our conception of the fixation process itself. Since, as we have seen, there is no true anatomical and functional central point of the retina, but only a more or less constant area of clearer vision, the traditional theory is robbed of the hypothetical point around which the motor organization could take place. Neither in the definiteness of motor impulses, nor in the definiteness of a fixation center are the necessary conditions present to make an exclusively motor organization of the retinal elements a tenable hypothesis.

[1] PSYCHOLOGICAL REVIEW, XI., 297–307.

Furthermore, as Lipps has pointed out with such vigor, the general conditions of the operation of the motor impulses in developing a differentiated organization of the retina, presuppose a degree of mental development and retinal differentiation which, if it existed at the beginning, would render the later development a useless and a gratuitous complication. Namely, there is only one motive for bringing a peripheral stimulation to the central area of the retina and that is lack of clear vision. Conversely there is only one final indication of the fact that the object of interest has reached the central retinal area and that is the relative clearness of the vision. But clearness and a lack of clearness are not primitive attributes of retinal sensations; they involve highly complex conditions of retinal organization and of perceptual elaboration. The only possible meaning to the clearness of a retinal impression involves the adequate discrimination of its parts, but until we have an organized retina, with a complete system of local signs, there will be no psychological parts of a retinal impression. Indeed not until there is considerable development of the total perceptual process can we speak of a unitary object at all. If it be objected that the tendency to bring the peripheral stimulus to the macula is an impulse independent of the matter of clearness then we must insist that such a hypothetical tendency independent of the matter of clearness must have died out in mature life, since it is conspicuously absent in the normal functioning of the eyes as illustrated by the process of reading. While if it really ever did exist it would indicate a pretty complete differentiation of the retinal elements antecedent to all movement.

A further difficulty in the traditional motor theory of retinal organization is the implication of the high degree of abstraction which it involves. Only rarely in primitive conditions would a visual field be composed of a single punctiform peripheral stimulus. As a matter of fact the typical retinal stimulus is a highly complex mosaic, all of which moves across the retina in consequence of the same eye movement that moves one particular peripheral point to the center of the fovea. But the point that actually comes to the center of the fovea cannot

be distinguished as a peripheral point without the operation of a considerable degree of abstraction from the surrounding stimuli, which in turn would presuppose a relatively high degree of retinal differentiation. As a matter of fact, except on the presupposition of an already organized retina, every motor impulse would be associated not with one retinal element but with the total changes throughout the whole retinal field, while the movement of the point previously at the fovea would tend to be rather more prominent than the obscure peripheral stimulus. The association of the movement with one single change is doubtful under the most highly developed conditions, but even if it obtained it would presuppose a differentiated apperceptive process which we have no grounds to assume in primitive conditions.

Finally, there is no ground either in adult life or in the observable eye movements of infants to assume that there is any motive to bring foveal stimulation to the exact center of the fovea. Indeed if such a motive had once obtained it must have perished before adult life. Records of adult eye movements indicate neither the motive nor the fact. As a consequence the motor theory of retinal organization even if it satisfied all the other difficulties which we have discovered, would break down absolutely at the very point where the organization of the retina is the most refined, namely, at the fovea itself. But any supplementary theory which would account for the foveal organization must also account for much more. I believe the conditions of such a theory of the organization of the retina are present in the general conditions of retinal stimulation, together with the involuntary fixation movements which we were able to demonstrate in the first chapter.

It is only fair to my general contention to point out at this time that the evidence against the adequacy of the motor local signs of the retina is not in any way immediately referable to the tactual local signs. Indeed I believe that we should carefully distinguish between the general character of the eye movements and the character of the other reactive somatic movements. The eye movements, in so far as they are integral parts of the visual process, are never the consummation of the process into which they enter. They are subsidiary to the

retinal and dioptric processes which they condition, and their chief function is to provide for a more adequate retinal stimulation. They are never intended as the final response of the individual to adequate knowledge of his environment; they never constitute his real reaction upon his environment. In this sense they differentiate themselves from conduct proper as belonging to a preparatory stage. They constitute action for the sake of knowledge rather than action upon knowledge. Doubtless the fact that they never constitute the real end of action has much to do with the relative inadequacy of their representation in consciousness. They contrast sharply with the movements of the head which in the lower animals almost entirely supplant the long eye movements that are observed in man. When a dog moves his head towards an object of interest his action is not merely preliminary to the clearer perception of that object, it is a part of his general adaptation to a situation. Whatever the new object of interest may turn out to be, he has executed a movement of strategic value and is in a better position to act upon any new intelligence which the movement brings him, whether it is the occasion for defence or for offence. This peculiarity of the function of the eye movements doubtless corresponds closely with their place in the conscious life. They are only partially voluntary, and are altogether withdrawn from voluntary control. Once they have begun, they can neither be hastened nor retarded, and we are impotent to change their direction. Finally, as we have seen, they yield only few, indefinite, and unreliable sensations of movement.

On the other hand we must not overlook the fact that the eye movements are closely related with the movements of the head and body through the general functional correspondence between the two, and especially through the coördinate compensatory eye movements. While the sensations of eye movement are conspicuously lacking, the motor mechanism for coördinating all forms of movement in response to particular needs are conspicuously adequate. They are much more refined than the coördination between peripheral retinal elements and the impulse that brings their stimulus to the fovea. So that, whatever function the eye movements may have in the

development of our visual space, the mechanism is already obvious that would serve to unite visual and tactual systems into an organic whole.

§ 3. *A Genetic Organization of the Retinal Elements.*

Notwithstanding the inadequacy of the sensations of movement and the motor impulses to account for the differential organization of the retinal elements, the fact remains that the latter are most delicately organized. This is true not alone in the so-called higher vertebrates, but also in the lower vertebrate eyes, where the mobility is much below that of the human eye. And there is no evidence that the organization of the retinal elements in the lower form of the vertebrate eye is any less complete than it is in man. Whatever the origin of the retinal organization is, then, it must be some primitive process, not too dependent on the movements of the eye, and not necessarily dependent on a high intellectual development. I believe there are factors in the most elementary retino-central processes, which must tend to give the retinal elements a physiological organization, corresponding point for point with that organization of the retinal impressions which we know in conscious life as spatial.

If, for the sake of logical completeness, we may postulate an absolutely undeveloped retina, a sort of *tabula rasa* affair, which, in its anatomical relations with the great nervous centers, is otherwise perfect, save in the matter of the mutual relationship and differential organization of its parts, it is obvious that the first stimulation by a complex visual field will produce a complex mosaic of retinal stimulation, corresponding with the general conditions of the dioptric apparatus and the anatomical arrangement of the retinal elements. But such a complex mosaic of stimulations would have neither unity nor a differentiation of parts; it would be completely unorganized. The differences of stimulation, however, must bring about certain changes in the different anatomical elements of our functionally undifferentiated retina. Any group of retinal elements *n* which is covered by the same patch of stimulus; merely by virtue of the similarity of the stimulation of its various

parts, thereby differentiate themselves in their life history from all parts of the retina which are otherwise stimulated. Normal complexity of the visual field will produce an indefinite number of such groups of elements, which are already differentiated in their physiological relationships from each other and from all other parts of the retina. But the eye is in constant motion. Entirely apart from all visual motives, we have seen how the purely physiological facts of pulse, respiration, and defective muscular balance must unite, even in a totally undifferentiated state of the retina, to produce a more or less continuous eye movement. Consequently, even if we admit no change in the mosaic of light and color, which stimulates the retina, the groups of retinal elements, which each part of the mosaic functionately associates, will suffer constant rearrangement. Each element in the first *n* group becomes in turn temporarily associated, because of the momentary identity in their life histories, with various other elements on all sides, which latter are in turn related to others. Thus, while the differentiation involved in the variation of the neurone life history constitutes a peculiar property for each retinal element, it also unites it, simultaneously or successively, with all other retinal neurones; immediately with its neighbors and mediately with the entire retina.

The process may be diagrammatically represented if for simplicity's sake we reduce it to the integration of a simplified

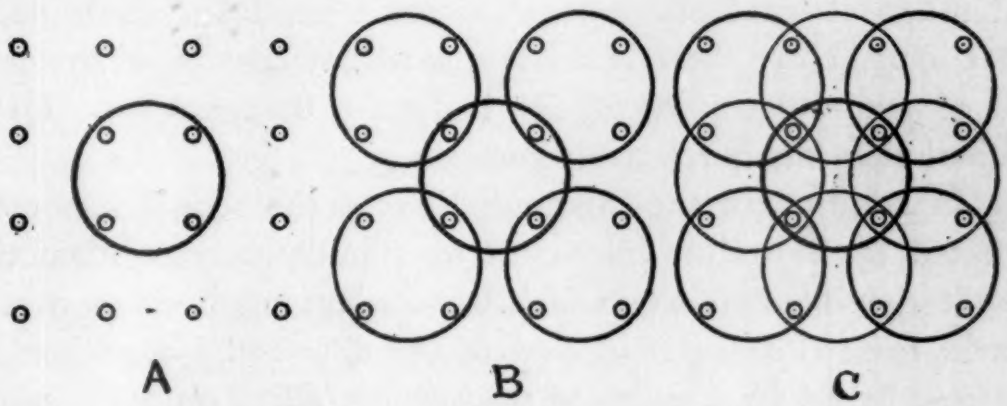

FIG. 4.

retina consisting of sixteen elements, and if we reduce the complication of the visual mosaic at the same time to a single patch of light, covering, at the first moment of stimulation, little

more than the central group of four elements, as in Fig. 4, *A*. Already, as the result of the first moment of stimulation, the central group will have become differentially associated, by reason of that peculiar difference in the life histories of the neurones which the stimulation effects. The result of involuntary fixation movements in all directions will yield a successive complication of the original groupings, corresponding to Fig. 4, *B* and *C*. A still better illustration of the differential organization would be made by a similar group of sixteen pegs, in which the differential organization might be represented by rings of twine.

The frequent repetition of these successive groupings must give them permanency. And the natural variations in the form of the stimulation must give the organization of the groups a refinement which is lacking in our first stimulation. Moreover successive stimulation must perpetually tend to unite into closer and closer functional relationship those elements that are more often similarly stimulated. And all the relationships so established are preëminently adapted to coördinate with the relationships of the tactual sense in the complex organization of our sensory experience which we have given in immediate consciousness as space. Whether any individual eye is ever forced to build up such an organization *de novo* is doubtful. The organization as we have described it is admirably fitted to be perpetuated more or less completely by an inherited anatomical organization. And the evidence is that long before there is delicate muscular control of the eyes or of any other part of the body the organization of the retinal elements is relatively complete.

In addition to the differentiation of the retinal elements, effected by objective differences of stimulation, doubtless the qualitative differences on which Wundt lays emphasis coöperate to the same end. But in view of the differential organization effected by group stimulation the burden falling on the original qualitative differences in the retinal elements is more nearly proportionate to their known differences. They would serve rather to differentiate and to associate relatively large concentric areas.

Moreover, pursuant to our general theory, it would be the physiological rather than the psychological qualitative differences that would come into consideration. The psychological aspects of the retinal organization are given in the spatial relations of the various parts of the field of view; and it is doubtful if the psychologically qualitative differences ever enter consciousness as local signs, or in any other capacity, until they are brought to light by the analysis of the psychologist. Common experience does not know the local differences of color perception, nor the hiatus of the blind spot; and it is difficult to understand how these qualitative differences could become operative under the most favorable circumstances except after a prolonged discriminative experience. Finally we are seeking the basis of that physiological organization of which space relations are the psychical correlate. The attempt to discover an identical fact in the two series seems to us as hopeless as it is useless. Success would not bridge the gap; it would only emphasize the differentiating elements and complicate the problem.

While the group differential organization of the retinal elements has been progressing certain gross differences must have been developing in the various parts of the retina. Since the differential organization depends on the definiteness and the complexity of the mosaic of stimuli, that part of the retina, on which the images formed by the dioptric apparatus are sharpest and consequently most numerous, will have a definite temporal advantage in its development over peripheral parts of the retina, and this temporal advantage in development will account for the origin of that more intimate connection with our cleared-up visual consciousness which later experience has abundant motives for maintaining. In this point again our genetic organization theory seems to me to have an advantage over the traditional local sign theory; since, according to the latter, unless we admit a certain amount of local discrimination at the beginning under the name of clearness, we must fall back on some otherwise unexplained inherited tendency to account for the tendency to move the peripheral image to the mythical retinal center.

Finally a further complication has been progressing. Along with the group differential organization of the retina, the whole system has been coördinated with a certain relatively late and relatively gross organization of the motor impulses. With every accidental eye movement there must have been established a bond of association between the changes of position in the group stimulations of the visual field and the concurrent motor readjustments which occasioned them. This association must tend to standardize the group organization at various parts of the retina; and, as a matter of fact, that is exactly the peculiarity which is most striking in the organization of the retina. Notwithstanding some significant variations like the differences in the upper and lower visual fields, that appear to me to resemble more nearly the complication of later experience rather than the result of the primary organization of the retinal elements, there is a remarkable unanimity in the spatial values of similar stimuli on different parts of the retina. This relative unanimity does not follow in any way, directly or indirectly, the laws of muscular strain. In order to be explained by the motor local sign theory the latter must invoke some complex standardizing product of experience, or else force the motor organization to subserve not only the local sign of a given retinal element but also the general change in the position of the entire visual field.

But the same associated motor organization that standardizes the different parts of the retina through its simultaneous concurrence with the changes at all points alike, may also serve as a connecting bond between the organization of visual and the organization of tactual elements. This again is directly congruent with the facts as we know them in the coördinate compensatory movements; and in the ability to roughly adjust the eyes with respect to any arbitrary displacement of the head, totally independent of any purely visual motives. It is congruent furthermore with the feel of the eyes for definite positions within the socket which is consciously associated, not with any particular point of the visual field as its local sign, but with the relative position of the area of clear vision and the whole visual field with respect to its visible boundaries and the

position of the head. In this respect our theory has a further advantage over the traditional theory, in that it leaves the motor organization unburdened by work which it could never adequately perform, and consequently free for the work which it very obviously does perform.

Finally the application of the traditional theory to the perception of a complex visual field such as obtains in reading imposes an uncommon tax on one's credulity. If it might be assumed for the moment that the conditions of absolute fixation which the traditional theory assumes were really a fact, then the fixation of a single complex letter like an *a* would involve the arousal of retinal signs in the form of differentiated motor impulses in every direction from the point of regard. But the motor impulses to move the eyes in every direction would produce a more or less balanced tension of all the eye muscles which must, in the nature of muscular innervation, not only be fatiguing, but it must produce marked irregularities of the muscular balance and consequently of fixation itself. When the visual field is complicated by the addition of other letters sufficient to make a word, each point in the new stimulus will be differentiated only by the motor local signs and the new groups of stimuli will increase the delicately modulated pulls on all sides, which must be further increased in complexity as well as in tension by the total stimulation of a simultaneously exposed page. A simultaneous presentation of movement in all directions is disconcerting enough. But the simultaneous presentation of an infinite gradation of movement in all directtions might well serve as an illustration of the utterly self-contradictory. While a corresponding increase of the tension of the eye muscles by a complication of the visual field is not a fact. The substitution of kinæsthetic residua for actual muscular innervation relieves the situation only with respect to the actual muscular tension. The self-contradictory absurdity of the attempted presentation still confronts one.

In contrast to this impossible conception appears the differential organization of the retinal elements in groups. The complex form of any particular grouping is a matter of entire indifference in the associated life histories of neighboring ele-

ments. And the complication of the visual field as it is found in reading instead of leading to a self-contradictory absurdity, must be regarded as only a slight increase in the complexity of the conditions that originally produced the physiologically differentiated retinal elements.

Appendix. The Technique of Recording the Eye Movements.

Since the final appeal with respect to the credibility of an alleged fact of eye movement, fixation, or muscular activity must be made to an objective record the technique of recording the eye movements becoms *ex officio* a necessary even if a subordinate part of our discussion.

The first reliable device for recording the eye movements was the mechanical registration apparatus of Delabarre.[1] Published simultaneously with the apparatus of Delabarre appeared the ingenious modification of the apparatus to meet the exigencies of recording the movements of the eyes in reading by E. B. Huey. In spite of its intrinsic worth it is improbable that mechanical registration by direct attachment to the eye will ever come into general use; partly because of the exactions of the technique, together with the extreme penalties of carelessness; and partly on account of the unaccustomed load which the eye muscles are compelled to carry. But Delabarre's plaster cup attachment has the honor of being the first really fruitful device to meet the new scientific requirements. Personally I cannot but regard it as a scientific misfortune that the elaborate material in his possession has not found fuller publication.

It was in consideration of the theoretical difficulties which appeared to be involved in registration by mechanical attachment, and before I was aware of Delabarre's success in overcoming them, that I worked out a plan for photographing the horizontal eye movements. Then and now a pencil of light appealed to me as the only available registering medium which had neither momentum nor inertia, which was absolutely safe and universally available. But the development of a satisfactory technique was a long process of experimental elimination and modification. The most serious of the early difficulties

[1] E. B. Delabarre, 'A Method of Recording Eye Movements,' *Am. Jr.*, Vol. IX., pp. 572–574.

concerned the lack of sharply defined optical contrasts on the eyeball. By a series of accidents and theoretical considerations in conjunction with the patient help of one of my pupils, T. S. Cline, Wesleyan, 1902, I came finally to use the reflection of light from the cornea. Unquestionably the most brilliant advance in connection with the photographic registration of the eye movements was made by Judd, when as the result of painstaking experiments he succeeded in finding an innocuous speck of brilliant white that could be placed on the cornea and photographed with a kinetoscope camera. Undoubtedly Judd's apparatus in its new form, giving practically a continuous exposure, is capable of getting accurate results under a greater variety of conditions than any other objective method now in use. I cannot but regard it, however, as a misfortune that with the general recognition of the high value of Judd's apparatus and the results he has achieved with it, there has developed a widespread but I believe an uncritical distrust of the method for which I feel a certain amount of personal responsibility and which for some kinds of work at least has I believe no superior. The criticism to which the method has been subjected, together with the use that others have made of it, that I am now making and plan to make, forces me to a formal discussion of its theory, the limits of its reliability, and its present technique.

After all the fundamental question with respect to any technique is whether it is serviceable. That I take to mean whether with intelligent use and adequate control, it is capable of extending our scientific knowledge concerning matters of importance; secondly, whether its incidental errors are capable of elimination or determination; and finally, whether, considering the results obtained, it is reasonably economical of time and laboratory equipment. It is by these tests that any method of registering the eye movements must stand or fall. No technique is responsible for carelessness or uncritical misuse.

When first published, my method was primarily adapted to measure the angle velocity of the eye movement. And until the present discussion, my own immediate use of the method

has been based rather on its reliability in measuring the temporal characteristics of eye movements than their exact spatial relations. But its ready adaptation, under proper control, to measuring the extent of eye movements as well as their duration led to an extension of its use. Doubtless some mistakes were made. Doubtless too the empirical control was not always adequate but such difficulties have confronted most methods with which I am acquainted. They would be poor excuse for announcing the obsequies of a technique or for too sweeping generalizations with respect to the limitations of its application, at least not until it has been given a fair experimental trial for its new work.

§ 1. *The Theory of the Movements of the Corneal Reflection.*

The theory of the method, briefly stated, is that the virtual images from an eccentrically mounted convex spherical mirror appear to move in the direction of the latter's rotation when its axis lies behind its center of curvature. Now within a very small error the surface of the normal healthy cornea is a convex spherical surface, as exact as the general visual process which it conditions. If the radius of the curvature of the cornea were infinitesimal, the apparent movement of the corneal reflection would equal the sine of the arc of movement measured on a great circle of the eyeball. If, on the other hand, the radius of the cornea were equal to the radius of the eyeball, and the latter rotated on its center of curvature, the corneal reflection would appear to remain stationary. As a matter of fact neither of the above suppositions is true; and the apparent movement of the reflection actually lies somewhere between zero and the sine of the angular movement of the eyeball. More exactly, since the average radius of curvature of the center of the cornea is 7.7 mm., and the distance from its apex to the center of rotation of the eye averages 13.5 mm., the apparent movement of a distant object reflected from near the center of the cornea will be slightly less than one half the actual displacement of the apex of the cornea but always in the same direction. More accurately, under the above conditions, the movement of the reflection will be $(13.5-7.7)/13.5 =$

(5.8)/13.5 of the actual movement, as may be proven by the following demonstration.

Let Fig. 5 represent the horizontal cross-section of a schematic eye situated before a camera and fixating successively points lying along the lines of regard *CL* and *CR*. Let the apparent source of light be at the center of the photographic lense. Then the only beam of light that will be reflected back to the lense from the cornea when the eye is in the position *CL*

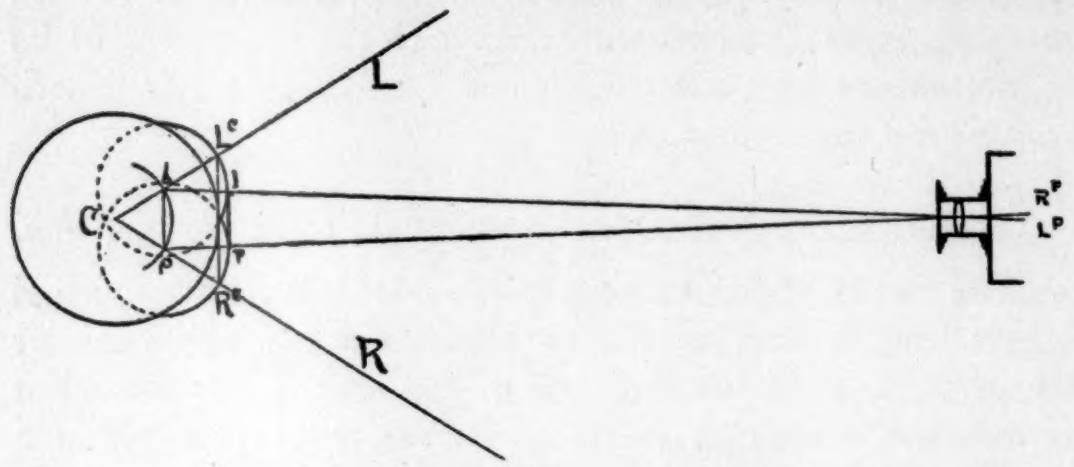

FIG. 5.

will be that whose line of reflection from the cornea is identical with its line of incidence, namely, that beam which if continued would pass through the center of curvature of the cornea. Consequently the apparent image of a punctiform source of illumination will be approximately at λ.

If the line of regard moves through the angle *LCR* the apparent position of the virtual image will change to ρ. Since as the cornea moves with the eye, the beam of light which will be reflected back to the lense from the new position of the cornea will strike the latter at *r*. But by the properties of similar triangles

$$\lambda\rho : L^cR^c :: C\lambda : CL^c. \qquad \text{Q.E.D.}$$

Somewhat unfortunately for the purposes of photographic registration the radius of curvature of the cornea increases slightly from its apex to its periphery, where it is highly irregular. It follows: (1) That no use of the corneal reflection is permissible which involves the use of the extreme peripheral and irregular portions of the cornea; (2) that all records

produced by eccentric portions of the cornea will be foreshortened in direct proportion as the virtual image seems to approach the edge of the cornea.

The most satisfactory relative disposition of the apparatus is that which confines the corneal reflection to the middle third of the cornea. Since, moreover, the middle third of the cornea is the part chiefly concerned in vision, accurate vision is a guarantee of a correspondingly accurate cornea. Only a small percentage or corneal astigmatism is corrected by the lens.

In actual use a direct mathematical elaboration of the results will doubtless seldom or never be necessary procedure. This is partly because absolutely accurate data with respect to the curvature of the cornea and the center of rotation of the eye are difficult to obtain in each individual instance. Partly, however, it is because of the comparative convenience of direct empirical control of the relation between actual displacement of the line of regard and the record. But it is also partly because of other sources of error which our method shares with all other objective methods and which resist easy mathematical evaluation. Probably the most important of these sources of error is the fact that the axis of rotation of the eye is not a fixed point, but a variable (see page 90). Since the actual displacement of any point at the surface of the eye during any given angular displacement of the line of regard depends on the distance of the point in question from the axis of rotation, it follows that any variation in the position of the axis during rotation will distort the objective record of the movement so as to make its various parts of unequal value. If the axis of rotation moved toward the retina, the extent of the eye movement, as measured by the displacement of any point on its surface, would be exaggerated. While if the axis of rotation moved away from the retina, the record would be relatively too small. This will be true of the cornea and its reflection according to the formula above demonstrated. But it will also be true of any direct attachment to the eye.

On these grounds, as well as on others to be exploited presently, it seems probable that the most satisfactory method of evaluating the records of eye movements must be based on the

correlation between certain known eye movements and their records. But under these latter conditions, considerable freedom is permissible in the relative position of the source of light and the general arrangement of apparatus. But it should not be overlooked that, for quantitative accuracy, the only theoretically ideal arrangement is to bring the apparent source of light to the center of the lens and to dispose the object of regard symmetrically about it. Any variation from this arrangement must be regarded as a concession to experimental expediency, and is permissible in quantitative work only under careful empirical control. These conditions of a satisfactory technique may at first sight appear to contrast sharply with the simple technique of Judd's Chinese white. It should be remembered however that we have been speaking of an optimum, and an analysis of the conditions will show an analogous optimum for the relative positions of the Chinese white and the camera.[1] While no method is perfect enough to safely dispense with the empirical control.

§ 2. *The Reliability of the Corneal Reflection Records.*

Under any conditions the question of reliability is a determinate problem, and its answer is the final test of the scientific value of a technique. My experimental answer to the question is based on a double experiment in which eye movements of known amplitude are compared first with the records by the corneal reflection and second with the records made by an artificial attachment to the eyeball.

In the elaboration of the test several difficulties were encountered whose solution has a general as well as a special value. The first difficulty was the general inaccuracy and instability of fixation which was discussed in the first chapter of this monograph. Since the illusion of fixation may persist while the point of regard wanders over an area whose diameter is a full degree of eye movement, and since this fixation area may be asymmetrically oriented with respect to the object of regard, there is no natural standard by which the accuracy of a registering device may be tested. But without some fixed point of

[1] See page 90.

orientation, objective records will merely duplicate part of the inaccuracy of the introspective illusions. This general difficulty is keenly appreciated by all who have given the matter careful attention. Judd, for example, frankly disclaims all implication of an inerrent correlation between any particular point of his record and any particular point of his object of regard. It was enough for his purpose to indicate accurately the form of the eye movement under varying circumstances. It is obvious therefore that my first difficulty is more significant than might be supposed from the immediate context in which it appears. And it is obvious, furthermore, that some solution of the difficulty is a fundamental condition of the reliability of any complete spatial interpretation of any eye movement records. Within a comparatively minute error, such a solution is found in the use of a concurrent after-image control of fixation during some part of the registration. While such control cannot prevent all wandering of the point of regard, it reduces its amplitude to approximately 10′. And under special circumstances the total efficiency of the control may be still further refined. The following test for example was concerned with a series of fixation in the horizontal plane. Vertical lines were chosen as the most efficient fixation marks. And a small round after-image at the fovea, projected in turn on the different lines, gave a delicate and, as the records show, a surprisingly accurate control. The minute fixation movements could not be entirely obviated. But after the line was once properly fixated they were comparatively small, and appear to have been symmetrically oriented. This resulted merely in an increase in the size of the corresponding record without disturbing to any measurable degree its center. One serious charge may be brought against the accuracy of this form of control, when the problem is to locate the absolute line of regard. Namely, it is questionable procedure to assume that the center of the after-image is bilaterally symmetrical with the exact center of the fovea. My answer is a plea of guilty. We cannot be sure of it. On the other hand, in the prolonged fixation of the object which produced the after-image, there is some ground for confidence that the inevitable wandering of

the eye, in so far as it effects the after-image at all will be evenly distributed about a central line which shall pass very near the center of the fovea. But whatever difficulties may be encountered in the use of the after-image as a means of general control, it was eminently satisfactory as a control of the accuracy of a series of six fixations in the horizontal plane, with which alone the present discussion is immediately concerned.

The second main difficulty in the experiment was to obtain an artificial pigment point on the eyeball, at once small enough and brilliant enough to give a record on a fixed plate, comparable with the sharply defined records which are obtainable from the corneal reflexion. This difficulty proved even more refractory than the first. Following Judd's lead and using his experience as fully as possible, I experimented with numberless arrangements which seemed likely to give satisfactory results. The final expedient is not ideal, but it was usable, and may be of some general interest. A local jeweler, Mr. Atwell, to whose interest and help I would hereby tender my public acknowledgment, finally succeeded in turning out and satisfactorily polishing a minute, almost microscopic hemisphere of silver. In order that this minute bead could be handled without getting lost, and also in order to give it an innocuous surface for contact with the eye membranes, I mounted it on a fragment of black tissue paper, approximately 1 mm. in diameter, which, following Judd's discovery, I had previously saturated in paraffine. The presence of this on the cornea, was absolutely painless, without the use of an analgesic. In general, it was entirely unfelt unless in winking it came in contact with the eyelid. This innocent report of its presence proved something of an advantage since it indicated the necessity of a readjustment.

Unless the eye is fairly dry however the whole affair tended to drop slowly. On this account I regard the device as less satisfactory than Judd's bit of Chinese white, whenever the latter is available. But it may serve a general purpose for fixed plate records and falling plate records, where the image of the Chinese white would be too faint or too large. As one might

expect the reflection from this almost microscopic bead, when it was in focus, gave the most delicately beautiful record I have ever seen. But unfortunately when it is not exactly in focus, the general diffusion reduces the photochemical effect so much that the bead for practical purposes requires a great deal higher illumination than does the reflection from the cornea.

The matter of this differential illumination was the third difficulty in the experiment. Since light intense enough for the bead was blinding to vision, and produced marked halation in the image of the corneal reflection, some form of differential illumination was demanded that would be intense enough for

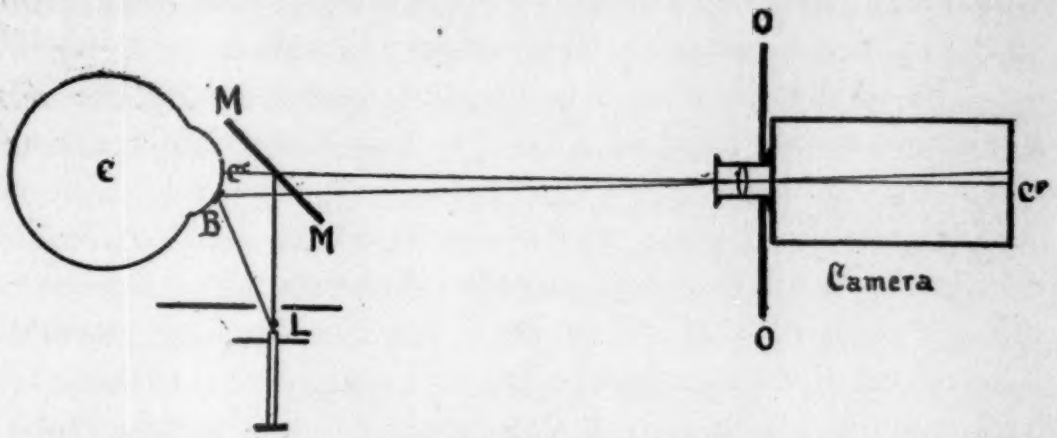

FIG. 6.

FIG. 6 gives a schematic line drawing of the arrangement of the apparatus. *C* represents a cross-section through the eye; *L* is the arc light; *MM* is the transparent mirror; *B* is the minute silver bead; C^c is the apparent position of the corneal reflection; *OO* is a sheet of white cardboard with the six vertical fixation lines.

the bead and greatly reduced for the corneal reflection. This could not readily be produced directly by confining the intense light to any particular zone, since the movements of the two objects left no exclusive zone covering all the positions of either object. The solution of this difficulty was finally effected by the help of my old friend, the transparent mirror. By placing the arc light about 90° to the right of the primary position of the head, the full intensity of the light would be reflected from the edge of the bead, without entering the pupil at all. While a plain glass plate interposed between the eye and the lense at about 45° to the line of regard, threw about one tenth of the

full amount of the light that struck it to the eye. But apparently its source was at the center of the photographic lense. A still further reduction in the apparent brilliancy of the light, was produced by the customary pot-blue glass screens interposed between the arc light and the transparent mirror. The resulting illumination was the mildest I have ever used. It scarcely provided a visible after-image of the arc for controlling the fixation, even after prolonged fixation. But it gave excellent records both on the fixed and on the slowly falling plate. Like the solutions to the two previous difficulties, the device is capable of general application. And I feel justified in recommending it for all quantitative records of the corneal reflection type in which it is desirable to bring the apparent source of illumination to the apparent center of the photographic lense.

The object lines were brilliantly illuminated by the relatively non-actinic light of a 16 c.p. incandescent light situated just above the head.

The resulting plate was a simple record of two lines of dots. As in all first-class records of the corneal reflection no other details of the eye or head are clearly distinguishable. The record alone stands out clear and sharp on an almost plain background. The record was first projected with a lantern onto narrow strips of white paper, which were pricked with a steel stylus as nearly as possible at the center of each fixation record. This gave a total magnification of the eye movements of about 80 times. The distance between the stylus marks was then measured directly by a scale. There are slight sources of error in the method of measurement but careful manipulation reduced them to a negligible quantity relative to the purpose of the test.

The results reduced to movements at the eye are given in the following table in fractions of an inch.

TABLE III.

	Left.		Center.	Right.	
	1–2.	2–3.	3–4.	4–5.	5–6.
	6°24′44″	6°41′14″	6°45′24″	6°41′14″	6°24′44″
Cornea A.	.02131	.02455	.02334	.02455	.021045
Bead B.	.051005	.05693	.05423	.05369	.04425
Theoretical C.	.0536	.0555	.055	.052	.047

Reading from above down the words *Left, Center, Right,* indicate the respective positions of the point of regard from the standpoint of the subject. 1–2, 2–3, 3–4, etc., indicate the movements between the respective fixation marks numbered from one to six beginning at the left. The line 6° 24′ 44″, etc., indicates the value of the respective eye movements in degrees, minutes and seconds measured on a circle whose center is the nominal axis of rotation of the eye. Line *A* gives the actual displacement of the corneal reflection in fractions of an inch, after all enlargement by enlarging camera and projection apparatus has been subducted. It is altogether reliable to three decimal points. Line *B* gives the actual displacement of the bead reflection from the bead in fractions of an inch. These are also accurate to three decimal places. Line *C* gives the thoretical values that should obtain in line *B* under the conditions of the experiment.

The interpretation of the table is neither difficult nor obscure. Doubtless the most striking feature is the relative accuracy of the corneal reflection data for similar angles on either side of the primary position. The records of the equal movements 2–3, 4–5 correspond absolutely. The records of the equal movements 1–2, 5–6 correspond to three ten-thousandth of an inch. The corresponding variations in the bead records are three parts in fifty, instead of none; and seven parts in fifty, instead of three in two hundred. The relative accuracy of the data from the corneal reflection needs no further demonstration. Under proper conditions it is as unimpeachable as any objective record, no more, no less. All the records show the absolute necessity of after-image control where strict accuracy is demanded. Thus it will be seen at once that in spite of the accurate bilateral symmetry of the corneal records, the movement 2–3 which should be slightly less than the movement 3–4 is actually greater by one point in twenty-five. Similarly the drop from the record of the movement 2–3 to the movement 1–2 is all out of proportion to the actual decrease in distance. These variations invalidate unilateral records as far as strict accuracy is concerned except under the control of some empirical standard like the after-image control. But these variations are not accidental.

Neither are they confined to the corneal reflection. Similar variations from the theoretically expected occur in the bead records at exactly the same places and in exactly the same direction, though in slightly varying amounts. Bead records 1–2 and 5–6 are smaller than the theoretical while bead records 2–3 and 4–5 are larger than the theoretical. In either series the records would indicate a real variation of the axis of rotation of the eye, of which the present records are a reasonably accurate indication. The occurrence of the indications coincidently in both records falls little short of demonstration. The data at hand show that the point of rotation of the eye varies approximately .02 in. in the movements under consideration. Doubtless considerably more exact measurements of the variation of the axis would be possible by the use of this joint method, but the present record is not available for that purpose, since neither the rigidity of my head rest, nor the accuracy of fixation, even with the best after-image control is adequate for measurements of the desired refinement.

The bare figures on the bead record taken in comparison with the bare figures of the corneal reflection method would seem at first sight to indicate that the corneal reflection method was the more accurate. As a matter of fact however any implication of comparative inaccuracy in the bead record would be altogether uncritical and unjust. With adequate mathematical interpretation neither has any decided advantage. It must be remembered that the corneal reflection records were made under optimum conditions for bilateral accuracy. A similar optimum for the bead was prevented by the latter's opacity, and the consequent interference with vision which would result if it were placed at the apex of the cornea. Practical considerations forced me, as they forced Judd, to place the bead in an eccentric position at the side of the pupil. In the present instance its actual position was 14° 49′ to the right of the apex of the cornea, about as near as was consistent with clear vision, and at approximately the same relative position that it has in the measurements of Judd and his associates. The simplest mathematical analysis must show that under these conditions, with the camera directly in front of the eye, the two halves of the record could not possibly be symmetrical.

Under the very best conditions if the bead were at the center of the cornea and the camera directly in front the records would be related to the points of regard as sines to tangents. But an eccentric bead never starts fair. When the line of regard passed through the fixation object at the extreme left of the series, the bead was almost directly in front of the camera. When the fixation was at the extreme right the bead would have moved unduly around, away from the camera, by exactly the amount of its eccentric position. Under these circumstances symmetrical displacement of the point of regard, with respect to the camera, will lead to an exaggerated displacement of the bead record at the left and a foreshortening of the record of movement at the right. In all the Yale records the distances were so short that these differences could doubtless be safely ignored, but they would obviously constitute a serious source of error under slightly different circumstances, unless they were subjected to empirical control. Indeed this is the most important lesson of the comparison. No method of objective registration is good enough to neglect either empirical control or theoretical elaboration where strict accuracy is demanded. No objective record can be safely taken merely on its face value.

But even under the most accurate control it is doubtful if we can expect to eliminate errors under 10′. Perhaps that is as accurate as our present needs demand.

§ 3. *The Technique of the Wesleyan Apparatus.*

In view of the considerable changes that have been made in our apparatus since it was first described, and in view of my belief that there is yet a real need for more, relatively accurate data concerning the eye movements, it seems worth while to add a working description of our apparatus in its present form.

The camera is an enlarging camera of fixed length. It is substantially a wooden box 4 ft. long and 6½ in. square, but tapering at the lens end. The lens is a Bausch and Lomb Convertible Protar Series VII., No. 8. Doubtless a cheaper lens would answer most every purpose, but the above has abundantly justified its presence in the laboratory. At the back of the camera box in place of the ordinary plate holder is an adapter

by which an ordinary double plate holder may be used for fixed plate exposures (Stratton's method); or my falling plate device may be instantly substituted at will without changing the focus, or otherwise disturbing the apparatus. This is a convenient feature and is recommended.

A crucial part of all registering devices is the head-holder. While that in possession of the Wesleyan Laboratory is fairly satisfactory, I have yet to find one that will hold the head absolutely rigid without discomfort. The only accessible point of contact with the skull is the upper teeth. All other points of contact are more or less compressible and elastic. But even the teeth are not rigid. For myself, generally, and in the above experiments, I use three points of contact: (1) An upper jaw rest, consisting of a strip of whitewood and in which a wedge has been cut which latter just fits into the indentation between the two upper incisors. The strip of wood is carried on a metal clamp, attached to a ½ in steel rod, which in turn is firmly fastened to the side suports of the head-rest. (2) The second point of contact is a wooden nose bridge, which is cut to fit the bridge of the nose, and is attached to a cross piece, which latter is clamped to the main uprights. (3) The third rest is a mastoid rest, consisting of a wooden block adjustable to various positions and capable of being firmly clamped to the main head-rest. I formerly used a mastoid rest on each side of the head but I found it awkward and of little additional help. This form of head rest is no guarantee of a motionless head. On the contrary it presupposes some intelligent control of the head and a consistent effort to maintain an even pressure against the three supports. That in spite of these precautions head movements of no inconsiderable amplitude still persist indicates the necessity of a head record.

Apparently the best illumination for the corneal reflection is the arc light. Following Holt's suggestion, I use an alternating current, and consequently get a serviceable time line combined with the record. The light is always stopped down by one or more thicknesses of pot-blue glass. This produces a highly actinic light of low physiological intensity. Such a light gives fine sharp lines on the negative and is always available. In both respects I find it better than direct sunlight which

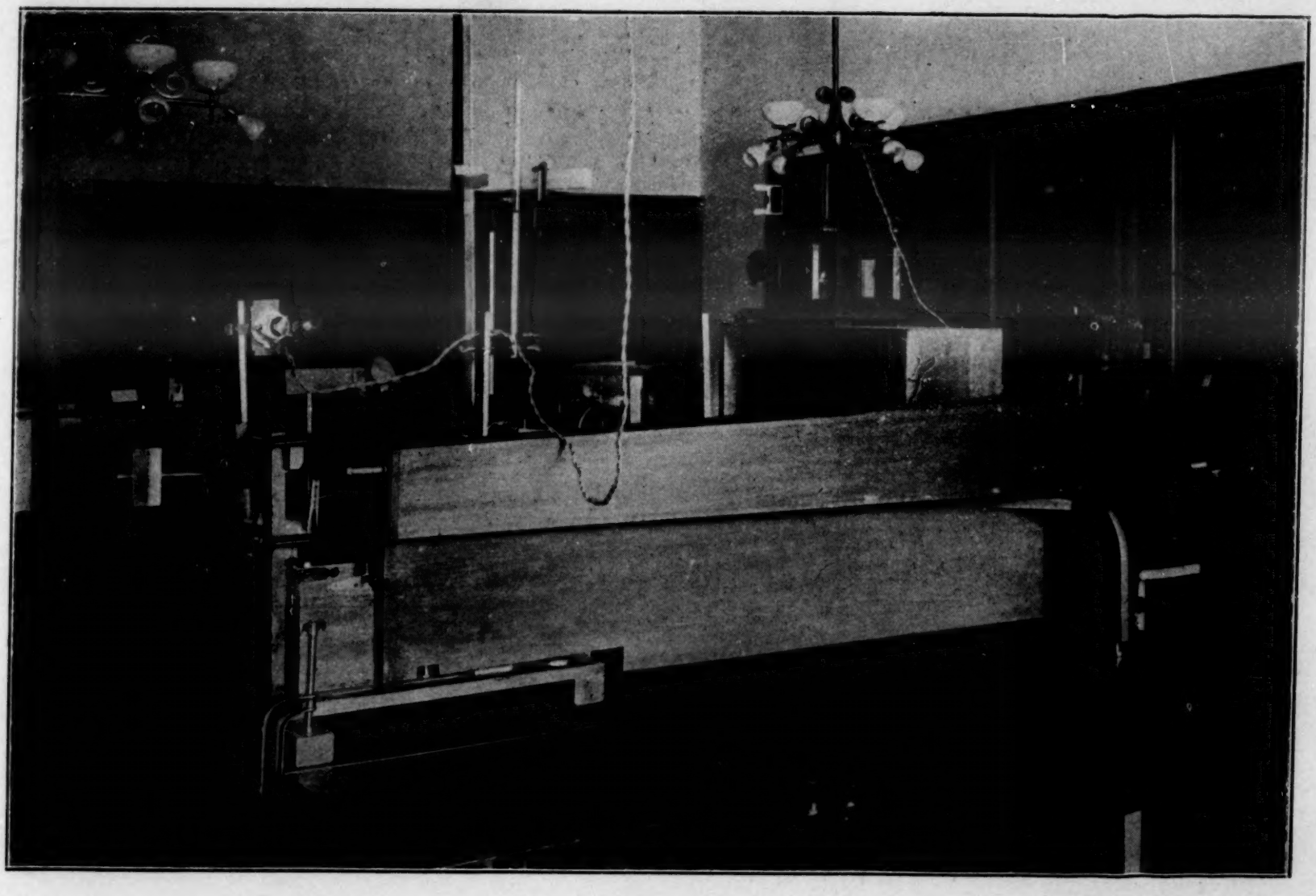

PLATE III.

PHOTOGRAPHIC RECORDING APPARATUS FOR REGISTERING THE EYE MOVEMENTS.

I formerly used. Undoubtedly the best arrangement of the light is to use the transparent mirror, described on page 87. Practically similar results may be obtained by the use of a small total reflecting prism directly in front of the lens and just below its free opening. Plate III. gives a picture of the entire apparatus in position.

We do not regard any record as wholly satisfactory unless the lines are minute hair lines. Big, black, broad lines look good to the naked eye, but do not permit accurate reading. The best lines do not show at all in the dark room and all the plates are developed by time.

Doubtless the most important control in the whole technique is the concurrent registration of the head movements. For this purpose I use a spectacle frame carrying small beads, much like Judd's. Under these circumstances the alternating current dots on a rapidly falling plate may on demand reach the definiteness of kinetoscopic pictures in which all the useless parts have been eliminated. The relative position of spectacle dots and corneal reflection dots will indicate vertical as well as horizontal movements. In the case of head movements the axis of the rotation of the head is so far behind the apex of the cornea that beads and cornea are practically one system for the minute movements of the head which are involved. The relation between them is illustrated by the lower part of Plate I., Fig. 2. Since the movement of the plate in this record was unusually slow to produce a long exposure the individual dots of the alternating arc are indistinguishable. But it is obvious that head line dots in connection with records like Plate I., Fig. 3, would give adequate data for triangulation readings. This device is a direct plagiarism of Judd's more elaborate kinetoscope pictures, but at present I have no adequate tests by which I can claim anything like equal accuracy in the registration of vertical eye movements on my falling plate. A momentary interruption of the arc light each second, by the rapid movement of a pendulum attachment greatly facilitates the reading of the plates by emphasizing corresponding points. In all cases where it was necessary to record the vertical movements of the eyes I have hitherto preferred Stratton's device of a fixed plate. The only difficulty in its use is the lack of both time and

head lines. The latter difficulty is a serious one but wherever a trained subject is available with special precautions to keep the head steady these records are reliable to 15′ as is demonstrated by the following control. With the plate moving so slowly that several minutes elapsed between the beginning of the experiment and the end I took a record of the fixations, with after-image control, of three series of movements across the six fixation lines, which served as fixation objects is the main test of reliability, page 85 fol. The last part of the record was the successive fixation of two of the lines five times repeated and this is reproduced enlarged in Fig. 7. All the fixation lines are crooked and show the fact, that was perfectly obvious by means of the after-image control, that the fixation was far from perfect. I persisted, however, in looking at the fixation mark until the after-image control indicated a moment of comparatively accurate fixation. That was the signal to move to the next line. So that the last moment of the record of any fixation was true as far as was possible under the circumstances. The successive voluntary displacements caused in general more irregularity than was present in the earlier test. Comparing the true fixation moments, which are represented in the reproduction at the top of each line, it was found that there was a slight displacement of the successive positions. These may be accounted for in some measure by the imperfections of the after-image control, but they are primarily head movements. In the fixations herewith reproduced the greatest error in any movement as represented by the variation from the mean was one twenty-fifth of the total movement, *i. e.*, 15′ of eye movement. The total displacement of the head was under 30′ and the maximum displacement for the entire unusually long series would give a maximum error in the readings if interpreted directly as variations in the position of the eye of less than a degree. This rather severe test may be taken to indicate the practical possibilities in the use of the recording apparatus without a head line control. Where the latter is permissibly dropped there is immense saving of time

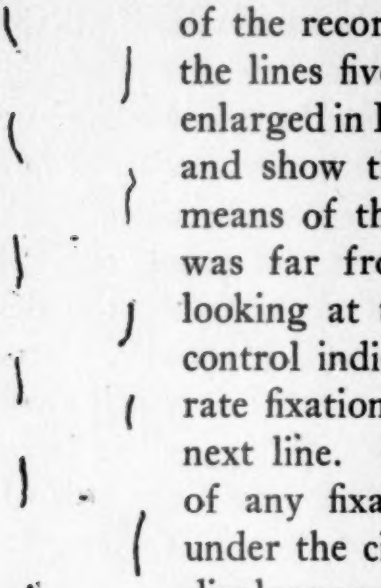

FIG. 7.

and plates, as a much larger number of records may be taken on one plate. In the extent of any given eye movement the probability of error will be below the error of measurement. In a succession of fixations the error ought not to be more than 1°. In general the maximum errors will be in the form of the slow drifts, which we observed in considering the relation of the head and eye movements. The smaller errors may be sharp and rapid corresponding to the pulse and respiration curves. Undoubtedly subjects vary in their control of the head movements and the variation in any given case should be tested out with the aid of the normal head lines.